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Does money buy happiness?

RICHARD A. EASTERLIN

For many Americans, the pursuit of happiness and the pursuit of money come to much the same thing. More money means more goods (inflation aside) and thus more of the material benefits of life. As it is for the individual, so it is for society as a whole. National economic growth—a steady upward march in average income, year after year, decade after decade—means, it is supposed, greater well-being and a happier society.

There have always been those who questioned, on moral or philosophic grounds, the identification of human well-being with material pursuits. More recently, the goal of economic growth has been increasingly questioned chiefly because of growing awareness of adverse side effects of growth, such as pollution and congestion. But the question of what the evidence is on the relation between money and happiness has remained, with a few exceptions, untouched. Are the wealthier members of society usually happier than the poorer? Does raising the incomes of all increase the happiness of all?

Doubtless, the main reason for neglect of this basic factual question has been the absence of data on happiness. Since World War II, however, reports on personal happiness have been obtained in a number of national population surveys here and abroad. Recently, I brought together, for a scholarly article, the data from 30 surveys

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conducted in 19 developed and less developed countries and analyzed the relation between reported happiness and income.[1] Because these data and this area of research are so new, the findings can only be considered tentative. But the results are provocative and the implications worthy of more general discussion—hence this brief summary for a wider audience. The concern here, it should be emphasized, is not with moral judgments about the view that money will make one happy. The question simply is: Given the prevalence of this view, do the facts actually indicate that people who get more money typically feel happier?

The conclusions to which the evidence points, in brief, are these. *In all societies, more money for the individual typically means more individual happiness. However, raising the incomes of all does not increase the happiness of all.* The happiness-income relation provides a classic example of the logical fallacy of composition—*what is true for the individual is not true for society as a whole.*

The resolution of this paradox lies in the relative nature of welfare judgments. Individuals assess their material well-being, not in terms of the absolute amount of goods they have, but relative to a social norm of what goods they ought to have. At any given time, those above the norm typically feel happier than those below. Over time, however, the process of economic advance leads, not only to a general increase in incomes, but also to a corresponding rise in the social norm by which material well-being is judged. This increase in material aspirations in a society, itself produced by material progress, thus negates the positive impact of income growth on happiness that one might have expected from the happiness-income relation among individuals prevailing at any point of time.

What is happiness?

Happiness is not, of course, simply a matter of material well-being, even to the "common man." When assessing their happiness—for example, whether they are very happy, fairly happy, or not so happy —survey respondents are frequently asked to explain what they mean by happiness. At first glance, one might expect this to produce as many definitions as there are individuals—and indeed there are differences in the replies. But more impressive is the striking similarity, not only within countries, but also among countries. If the replies

[1] Richard A. Easterlin, "Does Economic Growth Improve the Human Lot? Some Empirical Evidence," to appear in: Paul A. David and Melvin W. Reder, eds., *Nations and Households in Economic Growth: Essays in Honor of Moses Abramovitz* (Palo Alto, California: Stanford University Press, forthcoming).

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are grouped into broad classes, one finds that usually each of three concerns is mentioned at least a majority of the times in personal happiness judgments—economic matters, family considerations, and health, with economic concerns being most frequently cited. Other considerations, such as social or political conditions, are mentioned much less often—by perhaps only one person in 10, or less. On reflection, the similarity among people in arriving at happiness judgments makes sense. What concerns people most are the matters of their everyday experience. In all cultures the way in which most adults spend most of their time is the same—working, trying to provide for and raise a family, dealing with family problems and sickness. Hence the kinds of things people mention when asked about happiness are similar. Needless to say, within these general categories the specifics frequently differ. For example, while economic concerns are important to people in both developed and less developed countries, the particular goods they want are strikingly different, a point to which I shall return later.

Self-reports on happiness, then, are dominated by the "homey" concerns of everyday life. But is reported happiness credible—can people assess their own happiness? Will they tell the truth, or merely say what they think the interviewer wants to hear? These questions lead into technical matters in the evaluation of survey data which I cannot go into here. Those who have investigated these issues typically conclude that while there are biases in the data, the responses do bear a real relation to the respondents' subjective feelings. In particular, the data can be used to throw meaningful light on the question posed here of the relation between happiness and income.

The evidence on happiness and income

What do the data show on the comparative happiness of income groups within a country at a given time? Does greater happiness go with higher income? The answer is, quite clearly, yes. This does not mean that there are no unhappy people among the rich and no happy people among the poor. On the average, however, higher-income people are happier than the poor. For example, in a December 1970 survey of the American population, not much more than a fourth of those in the lowest income group (under $3,000 annual income) reported that they were "very happy." In the highest income group identified in the survey (over $15,000) the proportion that was "very happy" was almost twice as great. In successive income groups from low to high, the proportion "very happy" rose steadily. For the typ-

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ical individual, it would seem, more money brings with it more happiness.

This positive relation between happiness and income appears in every single one of the 30 national population surveys studied. Eleven of these surveys relate to the United States between 1946 and 1970, and 19 to other countries, including three communist nations (Poland, Yugoslavia, and Cuba) and 11 countries in Asia, Africa, and Latin America. Note that the association between greater individual happiness and more money appears without exception in widely different countries and social systems—in non-Western as well as Western societies, poor as well as rich countries, Communist as well as non-Communist nations. Happiness appears to be related also to marital status, age, and education; however, the influence of income is apparent independently of these other factors, and it seems to have the strongest and most consistent association with happiness.

This positive association with income is corroborated by the results of other studies of happiness and related emotional states. In an article published over 10 years ago, Alex Inkeles concluded:

> Those who are economically well off, those with more education or whose jobs require more training and skill, more often report themselves happy, joyous, laughing, free of sorrow, satisfied with life's progress. Even though the pattern is weak or ambiguous in some cases, there has not been a single case of a *reversal* [emphasis in original] of the pattern, that is, a case where measures of happiness are inversely related to measures of status, in studies involving fifteen different countries—at least six of which were studied on two different occasions, through the use of somewhat different questions.

In a comprehensive study surveying the American literature on mental health, James A. Davis reported that "study after study shows that mental health is positively related to socio-economic status in a variety of measures of mental health and SES."

The data thus show a statistical association between income and emotional well-being; the present discussion, however, has adopted a causal interpretation, namely, that higher income makes for greater happiness. A question may be raised whether the converse may not be true. Perhaps happier people are more productive and thus likely to make more money. It would be naive to suppose that the causal relation must run in one direction only. One reason, however, for thinking that there is a relation going from income to happiness appears in the respondents' own statements on happiness. As I have noted, when people are asked about the things that make them happy or unhappy, their responses indicate that economic, family, and health

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concerns are foremost. These concerns are expressed by both lower and higher income groups. The evidence shows, however, that the lower income group differs most from the higher in that the prevalence of worries about money is more frequent. This suggests that higher income, by reducing one major source of worry, is a cause of greater happiness.

National wealth and individual happiness

The repeated evidence of a positive point-of-time relation between happiness and income within countries suggests that if incomes increase generally in the population, there will be a rise in the average level of happiness. Is this in fact the case?

To answer this, we have available two types of data. One shows the historical trend for a given country in the average level of happiness as average income grows. On this, there is evidence for only one nation, the United States, since 1946. The other is point-of-time data for countries at different income levels. Since the populations of more developed nations have higher average incomes than those of less developed nations, we can ask whether their average level of happiness is also greater. Such international comparisons are available for several dates in the postwar period, the most comprehensive and intensive study by far being that done by Hadley Cantril for the period around 1960.

The showing of both sets of data—international and historical—does *not* conform to what one would expect on the basis of the positive relation between happiness and income prevailing *within* countries. Richer countries are not typically happier than poorer ones. In the United States, the average level of happiness in 1970 was not much different from that in the late 1940's, though average income, after allowance for taxes and inflation, could buy over 60 per cent more. By and large, the evidence indicates no relation—positive or negative—between happiness and national income. Whether the people in a particular time or place are comparatively happy is seemingly independent of the average level of income.

These results are not wholly unambiguous. The American data on average happiness, for example, do not fluctuate randomly over the 1946 to 1970 period as average income increases. Rather, they show a swing with a peak around 1957; by 1970 happiness has returned to about its initial level. This movement occurs among both higher and lower income groups within the country. With regard to the international data, it may be objected that happiness simply cannot be

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compared among different countries. I have already noted, however, that there is considerable similarity in the concerns expressed by people in different countries, rich and poor, when asked about happiness. Of course, even if there are cultural or other factors influencing reported happiness, this would not necessarily contradict the conclusion above on the absence of a relation between happiness and income in the international data. To establish that these factors are obscuring a strong positive relation between income and happiness in the data, one would have to argue that in poorer countries cultural or other influences operate systematically to elevate the level of happiness compared with that in richer countries—an argument which seems doubtful.

In any event, this much seems clear: In simple comparisons within countries, there is a repeated positive association between income and happiness; but when one compares rich and poor countries, or higher and lower income situations in a given country at two different times, the happiness differences that one might expect on the basis of the within-country differences by income level do not appear.

The underlying reasons

How can one explain this paradoxical result? If for the individual more money typically means more happiness, why not for society as a whole? The answer lies in the way that people form their welfare judgments. The satisfaction one gets from his material situation depends not on the absolute amount of goods he has, but on how this amount compares with what he thinks he needs. A lot of goods relative to perceived needs makes for a lot of material satisfaction and thus greater happiness; if goods are low compared with needs, the result is relatively less happiness.

A crucial question, then, is how are needs determined. If perceived needs were essentially the same for all people in all times and places, being determined, say, primarily by physiological considerations, then more money would mean more happiness, not only among individuals at a given time, but also in those societies which grow richer over time.

In fact, however, what people perceive as their needs is socially determined, and those who live in richer times and places perceive their needs in more ambitious terms than those in poorer societies. Needs, or material aspirations, are formed as the result of prior and on-going experience in a society—in the language of sociology, through the socialization experience of the individual. Thus what

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one "needs" as he reaches adulthood typically depends on the impressions he has formed of "how to live" from observing life around him and in his society while growing up. This is not to say that all individuals in a society conceive their needs identically. There are many personal and social reasons why differences exist; for example, peer group influences doubtless make for greater perceived needs among the rich than the poor, even in a given society. But because of the common experiences people share as members of the same society and culture, there is also considerable similarity in their perceptions of needs—there is, in effect, a social norm.

In general, then, the reason why higher income for a society does not typically mean more happiness is that the general level of needs, relative to which material well-being is judged, grows along with and as a result of income growth in the society. At any given time, however, the needs perceived by the members of a society are sufficiently similar so that those better able to satisfy those needs —that is, those with more money—are typically happier.

The extent to which perceived needs are governed by conditions in a society is emphasized by Cantril on the basis of his data:

> People in highly developed nations have obviously acquired a wide range of aspirations, sophisticated and expensive from the point of view of people in less-developed areas, who have not yet learned all that is potentially available to people in more advanced societies and whose aspirations concerning the social and material aspects of life are modest indeed by comparison.

In a comprehensive survey of long-term trends in American consumption, Dorothy S. Brady has pointed out that "[t]oday, the great majority of American families live on a scale that compares well with the way *wealthy* [emphasis added] families lived 200 years ago." But the typical American today does not consider himself wealthy. His consumption standards are not those of his colonial predecessors; rather they are formed by his personal experience with the human condition as evidenced in contemporary America.

The "hedonic treadmill"

The argument here, it should be noted, does not rest on the view that wants are consciously manipulated in the course of economic growth, though attempts at such manipulation doubtless occur. The point is rather that as the socialization experience of each generation embodies a higher level of living, so does each generation's perception of needs unconsciously increase. Even within the life

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cycle of a given generation, the progressive accretion of household goods due to economic growth causes a continuous upward pressure on consumption norms. This upward shift in perceived needs tends to offset the positive effect of income growth on perceived well-being that one might otherwise have expected. This mechanism is apparent in the results of a recent Taiwanese survey, analyzed by Deborah S. Freedman:

> While economic growth has increased income levels, it has, at the same time, expanded consumption possibilities with the result that present incomes seem no more adequate relative to needs. Only 20 per cent of the respondents said their financial position had improved during the last five years, although real per capita income increased about 40 per cent during that period.

Are we then trapped in a material rat-race, "hooked" on what Philip Brickman and Donald T. Campbell more euphemistically call a "hedonic treadmill"? The argument points to this uncomfortable conclusion. Each person acts on the assumption that more money will bring more happiness; and, indeed, if he does get more money, and others do not (or get less), his happiness increases. But when everyone acts on this assumption and incomes generally increase, no one, on the average, feels better off. Yet each person goes on, generation after generation, unaware of the self-defeating process in which he is caught up. To the outside observer, economic growth appears to be producing an ever more affluent society, but to those involved in the process, affluence will always remain a distant, urgently sought, but never attained goal.

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Other people's children: the day care experience in America

SHEILA M. ROTHMAN

VERY few recent proposals in social policy have received such enthusiastic and unqualified endorsement as the idea of day care centers. The chorus of approvals broadcast these past five years has gone almost unanswered. To a startlingly wide variety of proponents, the establishment of centers for the care of pre-school children seems like a good idea whose time has come. Such a reform, they contend, will beneficently reorder the existing relationships between sexes, generations, and social classes. Important support for the movement comes from women liberationists, who insist that day care will eliminate the most crucial barrier—that is, the day-to-day responsibility for children—which confronts women who try to enter the labor force and to pursue careers. Indeed, women liberationists argue that day care centers have actually fufilled this function in the past. World War II is their paradigm case: As soon as the federal government built day care centers, women entered the factories in unprecedented numbers. It follows that as soon as the government recommits itself to this program, women will again enjoy full opportunities for work.

But the argument for day care centers goes much further than this. According to many proponents, day care centers will do far more than simply allow women to join the labor force. They will

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once and for all resolve the conflict between the child's welfare and the mother's welfare—for the centers will offer advantages that neither the nuclear middle-class family nor the economically deprived lower-class family can match. According to day care proponents, especially the social psychologists among them, the day care centers will free the suburban child from the intense and overbearing pressures of the nuclear family, and therefore fit him better for an adult life in the outside society. For the lower-class child, the centers will offer exemplary medical and dental care, the chance to use the latest educational play equipment, and the skilled guidance of experts who will help the youngster cope with the deficiencies of his own family life.

Some day care advocates do concede that in an earlier era such child care institutions were frequently disappointing places. But the historical record does not give them pause. As Indiana Congressman John Brademas explained: "Traditionally it was easy to conceive of a child care program. The crèche and the orphanage are typical of what we used to expect: food, a roof over the head, and a minimum of adult supervision." But now, he insists, we know better: "In recent years psychologists have come to learn more about how children grow. As a result we have gained a deeper understanding of what intelligence, accomplishment, and development are." These ingredients will make the internal organization of our day care centers markedly superior to that of their predecessors.

Still another proclaimed purpose of day care centers is to bring a new cohesion to the social fabric, binding the social classes to each other. The center populations are to be mixed, so that lower-class and middle-class children, blacks and whites and chicanos, and their parents will all come into regular contact with each other. "Think for a moment what this would mean," Congresswoman Bella Abzug recently declared. "It would let local groups of parents and women set up child care centers for children from all socio-economic backgrounds. It would let community groups create the models for that universal kind of child care we are all talking about—a child care system that would accommodate rich and poor alike, that would let our kids grow up with a chance to know each other and to learn that they can bridge that racial and economic gap that divides their parents."

It is small wonder, given this definition of potentialities, that day care supporters called the centers the "*right* of every pre-school child in America." How can one deprive the child, the parents, and the society of such advantages?

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Still, before investing large sums of money in day care centers, we had better know what it is we are building. There is, alas, considerable evidence which indicates that this is a reform movement that will soon need reforming. In our long overdue effort to insure equal rights and opportunities for women, we may be establishing institutions that within a generation we will be doing our best to dismantle. It is not clear that day care centers historically have been an essential mechanism for getting women into the work force, or that they will become so. Nor is it true that enlightened change is the theme of day care developments, or that these institutions will bridge the gap between social classes. Rather, the history of day care in this country, from its origins in the 1850's through the events of World War II, is filled with cautionary tales that scarcely legitimate the kind of inflated rhetoric that we are now hearing.

The beginnings of day care in America

The first American day care center—or day nursery, as it was then called—opened in New York in 1854, the work of a group of the city's wealthy ladies. They were going to do good; specifically, they were going to assist impoverished and unfortunate women through the trauma of giving birth—and simultaneously satisfy a pressing need of their own, to obtain wet nurses for their infants. The plan was ingenious: It called for a combination foundling home, lying-in center, day care center, and employment bureau. The poor or deserted or unwed mother came to the center in the last months of her confinement. She had her baby there, and then, as recompense for the charity she received, she nursed not only her own infant but one other one as well—either a foundling or the infant of an employed wet nurse. After three months, if her milk was adequate and her behavior proper, she faced a choice of accepting a position as wet nurse or taking her infant and leaving the center. If she chose to be a wet nurse, she left her own infant in the day care center (to be fed by another three-month resident), and went to work in the homes of "the better sort." Everybody's needs, it would seem, were satisfied. The poor had a place to have their babies, and a ready means of employment. The wealthy had wet nurses, the gratifications of benevolence, and the chance to teach the downtrodden the right rules for living.

The essential characteristics of this first day care center reappeared time and again in later and more extensive programs. Throughout the 19th century, and well into the 20th, it was the

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elites that organized and administered the centers. It was their definition of proper behavior that went into the rules and set the tone for the enterprise. Moral uplift was endemic to the venture; the ultimate purpose of the centers was not merely to allow poor women to earn income but to bring them and their families under the "right influences." The centers tried to encourage female employment in the "socially desirable" occupations—that is, in domestic and not factory work. There were other day care models that Americans *might* have emulated. In France, for example, beginning in 1846, a series of crèches were constructed right next to the factories. Their chief aim was to allow the laboring mother to take brief work breaks in which to nurse her child. While not designed to "liberate" lower-class women, the French system did in fact operate in accord with their economic needs, rather than in accord with the moral needs of those who established and operated the American centers.

In the closing decades of the 19th century, as foreign-born immigrants crowded into America's urban slums, the day care movement spread in an ad hoc way from one city to another. In Boston, New York, and Chicago the benevolent-minded established centers. With the zeal and confidence of missionaries to the natives, they leased the bottom floors of brownstones in the midst of ghettos and eagerly, if haphazardly, set to work.

Their biggest prize, of course, was the child. They had him in their control at least during the daylight hours, and they were not going to waste the opportunity. As one day care organization explained with all due sympathy and condescension: "His mother must bring him in clean in the first place and her primitive methods are supplemented by the skilled service of competent nurses. . . . He is regularly fed, and his weak puny body soon shows the result in greater strength and better brain power. . . . He is taught good manners and pleasant ways." In short, he was to be transformed into a strong, intelligent, respectable child who would become a law-abiding and hard-working American citizen.

This child was also to serve as guide and teacher to his own family. "Nursery training," as one center official put it, "brings good not only to the child but to his home." Under the tender attention of the day care center workers, for example, he was to learn affection. When he returned home at night, he was then sure to show his parents love, and they in turn would reciprocate. So, too, his cleanliness and good manners were to inspire them to emulation.

Moreover, the day nurseries of this period devoted considerable energy directly to the uplift of the mother. They made efforts to

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find her domestic work, certain that the moral refinements of a proper household—with the example of how to wash, sew, clean, and cook constantly before her—would do much more for a girl's welfare than the factory. Some day care centers organized night classes for women to learn to do laundry and housework, in order to make them even better mothers and more marketable domestics. There were classes, too, for cooking and sewing, designed to improve both the children's meals and the family income. Some day care centers also included school-age children in their programs. The nursery took care of the youngest, the kindergarten of the next oldest, and afternoon clubs and playground groups of the oldest. After all, the lessons that the toddlers learned were just as relevant to their older siblings. So from 6:30 in the morning, when breakfast was served, till 9:00 at night, when the last mother departed, the day care centers tried with energy and good will to pass on their special brand of values and skills.

The settlement houses

It was reformers in the Progressive period who first attempted to "professionalize" day care, to make the centers' programs an integral part of a broad social welfare policy. From storefronts and church basements and vacant rooms in boarding houses, the centers moved into many of the new settlement houses. Young and dedicated college women now supervised the children, replacing the well-to-do matrons who had given of their Thursday afternoons. The settlement workers designed a systematic program of education, medical care, and mental hygiene to bring coherence to what had been an odd assortment of programs.

The Progressives' agenda for the centers was impressive. They were to be not merely custodial places where mothers could drop off their children on the way to domestic work, but innovative nursery schools. Children from three to six years old were to learn under the Montessori method; the centers at the same time would train kindergarten teachers in the newest pedagogical techniques. Moreover, day care would provide access to medical facilities. Children would receive regular physical and dental checkups, and in cases where emotional disturbances appeared, mental health advice too. A social worker was to investigate each day care applicant, obliged not only to inform the parent of the various facilities available but to administer social case work assistance where necessary. The settlement house also tried to integrate older children into its

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program through drill clubs, reading clubs, and discussion clubs, and to service mothers through instruction in English, crafts, and homemaking. "Last year," one day care center in the Progressive period boasted, "we received 1800 children. We gave many of them more abundant food and much better care than their poor homes could afford." Under the promise of this program, centers grew very popular. Two hundred six licensed day care centers operated in 1905, 500 in 1912, and 695 in 1916, the high point for the movement in the pre-World War II era.

For a brief time, some of the well-run settlement houses actually seemed to come close to realizing their ambitions. Hull House in Chicago and the University Settlement in New York for a few years did provide a wide range of services to the children of hard-working immigrants. But in pretty short order, probably by the outbreak of World War I, and certainly by 1925, conditions in the day care centers deteriorated. Inadequate personnel and custodial care, along with a rapid turnover in staff and clientele, characterized their operations; concomitantly, they grew less popular, the number of licensed centers declining from 695 in 1916 to 600 in 1925. Like so many other social welfare institutions, they enjoyed a brief spurt of seeming excellence and then suffered a prolonged lapse into mediocrity, or worse.

From second choice to last resort

One cause for this rapid decline was the competition that came from another Progressive reform, the widow-pension movement. Social workers and settlement house leaders vigorously insisted that morally deserving widows or mothers with physically disabled husbands should receive adequate relief at home, and not be forced to leave their children in order to eke out a living in factories or sweatshops. In fact, most reform energy went into the framing and eventual passage of such pension legislation. The Progressives' priority was to keep the good mother at home with her children, not to supervise the children in centers while the women worked. In other words, day care had a quality of second choice about it from the start. Progressives defined the program, for all its potential benefits, as less desirable than proper family care at home.

As a result of this judgment, and of the passage of liberal legislation, the day care clientele quickly became stigmatized, in their own eyes and in the eyes of center administrators. They were the unworthy poor—the mothers of illegitimate children, especially;

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otherwise they would be at home with their children, living on a pension which was now available. The decision to take on factory work had a dubious quality about it, and by obvious extension so did the decision to place one's child in a day care center. Not surprisingly under these circumstances, the centers soon had more vacancies than children. Within a decade, they moved from second choice to place of last resort.

An unbreakable chain of events spiralled the centers downward. As the quality of the care declined, more vacancies were created; as more vacancies occurred, funding became more restricted; as funding declined, the quality of administration fell and the stigma associated with day care centers increased. Then the level of care dropped further, more vacancies occurred, and the cycle began over again.

Thus, within a decade the day care center operations bore little resemblance to the plans of their founders. The clientele was no longer composed of the children of hard-working immigrant mothers who, following the reformers' advice, did domestic work during the day and attended sewing classes at night. Instead, illegitimate children, along with those whose mothers had no choice but to work in factories, now made up the bulk of the centers' children. These types had not been welcomed in the first Progressive day care centers—but by the 1920's the centers were glad to have anybody to service.

Also, rapid turnover of both children and staff now became typical. Pennsylvania charity officials estimated in 1923 that 6,000 to 10,000 children passed through its 61 day care centers in that year. Under these conditions, there was little left of a coherent program. Evening classes ended; Montessori techniques in schools disappeared. The centers became places to care for the children of the most unfortunate or incorrigible of the poor.

Investigations of day care conditions revealed all too clearly the woeful effects of these changes. In New York, the Public Health Committee of the New York Academy of Medicine examined the 112 licensed centers of New York City. Their findings: Less than half provided the most elementary kind of sanitation or safety; 14 per cent were totally unfit for use. In Pennsylvania, Helen Tyson, a social worker and prominent state charity administrator, examined the 61 licensed day care centers. Her findings: The centers were "a catchall in cases of desertion, non-support, unemployment, sickness, etc." The matrons in the centers knew almost nothing about the children—after all, as one worker told her, "the children changed

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so fast that she could not keep a daily roll book." Over half the centers did not meet basic standards of cleanliness. In most centers the children's play was undirected, the equipment was poor, education was totally absent, and general supervision was inadequate.

The new social workers

Yet despite all these disappointments, the idea of the day care center as a viable and important means for social improvement did not die out. Between 1925 and 1930, the newly professionalized social workers took hold of it, eager to turn the centers into places that would bring case work techniques to bear on the "pathologies" of the poor. Miss Tyson herself was more impressed with the promise of case work than with the dismal reality she uncovered. After recounting her findings, she concluded her report by insisting that new social work techniques, together with better licensing procedures and more demanding standards, would transform the centers. "The best nursery," she insisted, "is superior to the home." Or put another way: Highly trained social workers could now do a far better job at raising the children of the poor than the poor themselves could do.

The new model for the day care centers was the child guidance clinic. Hence, far from being reluctant to admit the children of the "unworthy poor," as their Progressive predecessors had been, the social workers of the 1920's focused precisely on the product of the "unstable home." From their perspective, personal as well as social problems had their origins in psychological maladjustment—if only the poor could be cured of their emotional disturbances, then they and the system would benefit. At the heart of this effort was to be the day care center; in it children and parents together would be taught the right rules for living.

The social workers were to be the central figures in the day care operation. They would administer elaborate admissions procedures to reduce the number of clients, and would intensify the relationship between the families and the nursery. The social workers would hold numerous interviews with the parents, and then channel information between the nursery teacher and the family. The teacher would learn all the important features in the child's background—how he had been raised, his family's psychological difficulties, etc.; and in turn the family would learn more about the child—his strengths and weaknesses, and how to help him overcome his troubles. The interviews would also give the social workers the

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chance to do case work therapy with the family and to use their new skills to improve "adjustment" and "functioning." In essence, the center social worker would promote family and community stability, bringing a new order to the lives of the poor.

For all the novelty of the language and terminology, this program was little more than an updated restatement of the most traditional viewpoint on the poor and on day care. The founders of the earliest centers had also vowed to inculcate the proper habits in lower-class parents and children, and so had the Progressives. To be sure, the nature of the clientele in the 1920's had changed; social workers were now eager to work with the "hard core," previously thought to be incorrigible. But their approach and their goals remained the same. United Charities, an organization devoted to the very traditional ideal of bringing efficiency to almsgiving, was attracted to the social work ideal and made day care an essential item on its list of causes worthy of support. So, by the close of the 1920's, centers were once again beginning to grow in number and in reformist appeal.

Before social workers could capture the movement, however, the Great Depression intervened. The first years following the Crash bankrupted most private charities, making it impossible for organizations like the United Charities to support existing centers, let alone expand them. Moreover, by 1933 the bulk of professional social workers had been pressed into government service to staff the relief and works agencies. Finally, the labor market for women almost completely disappeared. In WPA projects and in private industry preference went to men, on the assumption that they had to support families. A nation suffering so keenly from unemployment did not want to employ wives or single women. So with clients, supporters, and staff occupied elsewhere, day care centers limped through the 1930's.

The one exception to this general condition during the Depression was the founding, under the auspices of the WPA, of some day nurseries. These places were to provide unemployed teachers—particularly the women driven out of the private labor force—with a chance to work, and to give children some free food. As with most WPA projects, the nurseries were a temporary expedient, a stopgap measure until teachers could return to the school system. The children who attended came from families on direct relief or on WPA rolls; the mothers were almost always at home themselves, but the family's resources were not above subsistence. The function of the WPA nurseries, then, was not to educate the child, or to free the

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woman for work, or to bring social case work to the families, but simply to provide jobs to teachers and hot meals to poor children. As one would expect, turnover in both clients and staff was very high, facilities were inadequate (the basements of schools or public buildings), and the entire effort was really a minor venture in relief giving.

The War and its aftermath

The next great national emergency, World War II, had a very different effect on the number of day care centers, but not on their mode of operation. With men serving on the war front, and production a critical goal, the country had little choice but to press women—married, single, or widowed—into service on the home front. Occupations which had been traditionally closed to women suddenly were opened and women in unprecedented numbers filled the labor force.

Within a year of Pearl Harbor, these circumstances forced the federal government to enter the field of child care. The government wanted to maximize the number of women who could work, and hence it made good sense to provide facilities where mothers could leave their children during factory hours. It was also important to avoid demoralizing scandals. During the first year of the War, it was not uncommon to find children locked in parked cars near a factory—surely some arrangements could be made to avoid that kind of "care." Finally, the government wanted to allay widespread fears that the influx of so many mothers into the work force was to the detriment of their children.

Accordingly, in 1942 (under the Lanham Act) the federal government appropriated funds for the care of pre-school children. On a fifty-fifty basis, it matched state funds for organizing or expanding day care facilities. The growth of centers that followed this act was incredible; practically every community heavily involved in defense work ran one. Under the press of this emergency effort, as might be expected, the centers themselves were of makeshift quality. They had only to be better than a locked car, and by that criterion they did succeed. Set up wherever a few rooms could be had, they fed the children, kept them from harm, and allowed their mothers to work.

But these were temporary efforts—indeed, influential organizations worked to insure that this was so. A 1942 day care conference organized by the Children's Bureau quickly decided that the centers

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should *not* be constructed right next to the defense plants. For then they might be *too* convenient—the centers might outlive the emergency and encourage women to stay in factory work after peace had come. Hence, to no one's surprise, as soon as the War was over, the federal government stopped funding the centers; and with the exception of California and New York, so did the states. To most observers this meant that stable and middle-class mothers could get back to doing what they should be doing—raising their own children.

In the decade and a half that followed World War II, a new group of women, ignoring traditional injunctions, began to enter the labor force. To an unprecedented degree, not only the very poor but also lower-middle-class and middle-class women began to work; and they held clerical and sales positions, not only factory jobs. At the same time, the number of working mothers increased. In 1948, only 20 per cent of the women in the labor force had children under 18; by 1958, the number swelled to 30 per cent. Yet these two changes had little or no effect on the day care movement. Of the 5.1 million children under 12 whose mothers worked full time, only two per cent received group care; for those children under six years of age, the figure was a mere four per cent. What day care centers there were, were grossly underused; they operated at most at two-thirds capacity.

In other words, the first major influx of women into white-collar jobs took place *without* the assistance of day care centers—and without any public demand for such assistance. In fact, the service agencies and the public at large continued to define day care centers as appropriate only for "problem" children, those from "marginal" homes, in a psychological or economic sense. Well into the 1960's, hardly any observers valued group care over the home. Rather, they defined the centers as appropriate only in those cases where the mother might otherwise abandon or give up the child. Day care was a last ditch effort to keep the mother with her child.

The day care revival

And so the matter rested until the late 1960's, when a curious combination of forces gave day care a totally unexpected and unprecedented stimulus. The first groups to revive the day care idea, albeit from a different perspective, were composed mainly of educated women from the middle and upper classes. They perceived day care as a very useful method for facilitating the advancement

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of their own careers without sacrificing their children's welfare. But however worthy their ambitions, one must treat their solutions with a great deal of caution and skepticism. There are many sound reasons for questioning the psychological and educational theories with which these women have rationalized their advocacy and use of day care centers. But in a fundamental sense, the long and intense debates over the virtues of "insular" nuclear families as opposed to group care, over the wisdom of early as against later schooling, are not of primary significance to public policy makers. For the decision of a middle-class or upper-class mother to send or not to send her child to a day care center is ultimately a voluntary one. Among the well-to-do, those women persuaded of the one argument will use the centers; should they grow disappointed with a particular institution they can remove their child, place him in another, or hire a baby sitter. They have discretion and power; they act as free agents, choosing their side and taking the consequences.

But the rhetoric of these day care center advocates has much more serious repercussions for a very different clientele—the poor, particularly the welfare mother. Perhaps the rise in relief cases over the past decade would, in and of itself, have prompted a revival of interest in the day care solution. What is incontrovertible, however, is that the women's liberation rhetoric justifying day care has coincided with municipal and state pressures to curtail the numbers of mothers on relief, to get the "shirkers" off the rolls, to restrict Aid to Dependent Children, to make welfare mothers enter the job market rather than remain at home with their children. For those persuaded of the propriety of these measures, the legitimation of the day care idea could not have come at a better time. Here is a socially acceptable mechanism for getting welfare recipients back to work, and at the same time for transferring the care of their children to "better" hands. There is no group that reads women's liberation literature on day care with greater pleasure than relief administrators. The results of this coincidence do concern public policy—for here, to an unprecedented degree, we enter the area of compulsion and manipulation. Until now day care centers, for all their inadequacies, were typically part of a voluntary and private charity effort; the poor did not have to use them. Today, however, we are entering a new stage, wherein the government, under the threat of withholding relief, can compel the poor to put their children in the centers.

The project that most unequivocally demonstrates the coercive

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potential in linking day care with welfare is the federally sponsored Work Incentive Program (WIN). Established in 1967 by Congressional legislation, WIN was ostensibly designed to provide welfare recipients with the training and skills necessary to get them jobs in the open market. First priorities in the WIN program went to men, since Congress insisted that they were to be the first helped in getting jobs—or, put another way, the first removed from the welfare lists. After two years of operation, however, WIN had little to show for itself. The men on welfare, it turned out, were usually unable to work because of physical and mental disabilities; WIN then turned to women—that is, to mothers receiving ADC. Here they hedged their bets in an eminently predictable way. Recognizing that the likelihood of putting many welfare mothers to work was low, they took refuge in a much more ambiguous and hard-to-measure promise of educating and improving their children. "The WIN program," announced officials, "will be judged not only by the extent to which it enables mothers to obtain employment, but also by its performance as a program serving the welfare of children."

The structure that was basic to this effort was the day care center. Because of the new-found legitimacy of group care, legislators in state after state could, for the first time, *compel* needy mothers with children under six to enter the job market via the WIN program. Their rationale, of course, rested on the superiority of the new day care centers; once there was no need to be worried about the child's welfare, there was every reason to insist that the mother earn her keep. Thus the original purpose of ADC, to keep the family together, could be contravened.

The tangible results of this effort, as might be expected, have been disastrous. WIN has proven to be a very costly program, unable to get ADC mothers into the job market, and unable to provide first-rate child care. But WIN has probably succeeded to some degree in its more fundamental purpose—to frighten potential welfare recipients from applying for relief.

A recent evaluation of WIN, commissioned by the program itself, points to its key failures. For one, despite state laws that welfare mothers could be made to enroll in WIN only where "adequate" child care facilities were available, the great majority of WIN mothers were left completely to their own devices in obtaining child care. For another, the costs of providing "adequate" centers turned out to be *very* high. "It costs more," the evaluation noted, "to provide quality day care to children than most states are willing to pay mothers [under ADC] to take care of their own children." In effect,

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then, the WIN program "may result more in a transference of funds from the mothers to child care vendors with little reduction of actual costs." In other words, the welfare mother must get off ADC to earn money to help pay the salaries of middle-class teachers, social workers, and psychologists. *They* get perhaps $8-10 an hour for taking care of the children; she got perhaps $1-2 an hour on welfare for doing the same thing.

The WIN evaluation also highlighted another major flaw in the program. The welfare mother who did in fact gain skills and entered the job market could leave her child in WIN day care centers for only three to six months. After that, she was on her own, compelled to pay all the costs of child care. But the working mothers would probably not earn enough to cover day care costs, or even after-school or summer programs for older children. These circumstances left little incentive, indeed little opportunity, for the mother to leave welfare for employment.

Promise and reality

The WIN experience is all too typical of day care center ventures: They do not fulfill their promises. In January 1972, New York State initiated a day care center program based on the federally-approved sliding scale of payments; the scale was to enable all classes to take advantage of day care facilities, paying according to their needs, and at the same time to keep the cost of the program short of the astronomical. In effect, however, what it did was to subsidize center care for the poor and place public facilities beyond the means of almost everyone else. Those families earning under $4,320 per year received free day care center care. But a family of four earning $8,500 had to pay the full share, that is, $2,700 per year *per child.* For that price, a family could send its child to the most expensive New York private school, and still have money left over to pay the costs of a bus and some baby sitting too! More to the point, no family making that salary could afford those payments. Thus, the effects of the New York program are twofold. All but the very poor are priced out of public day care. And for the very poor, as one angry mother put it: "Rockefeller has made day care centers into concentration camps for the poorest black and Puerto Rican families."

The New York experience is not at all unusual. A study of 500 centers in all regions reported that fully 75 per cent of them are essentially segregated, serving either whites or blacks exclusively. This is a far

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cry from the day care center as "the meeting ground" of rich and poor, black and white!

The persistent failure of licensed centers to achieve or maintain adequate standards of care is even more disheartening. In fact, the reality of public day care as it now operates is dreadful, and there is no prospect that conditions will improve substantially in the future. Practically every systematic investigation of day care operations reports the same dismal findings. A 1972 study by the National Council of Jewish Women of 500 centers, selected at random from all parts of the country, concluded that the level of care was far lower than what parents had the right to expect. Of public, nonprofit day care centers, 62 per cent provided nothing better than custodial care; of proprietary, privately-run centers, 85 per cent were just custodial. Only 10 per cent of public centers and one per cent of the proprietary centers gave "superior" care—that is, the kind of care the rhetoric of the centers has promised. So too, a recent survey in New York City of its 240 licensed centers found 52 "inadequate," in that they did not meet minimum standards of health, sanitation, and safety.

Part of the failure of centers to offer good care reflects the inability of municipal or state governments to devise mechanisms for enforcing standards. To date, officials have relied exclusively on licensing, a woefully inadequate procedure in this instance. As the National League of Women Voters of Minnesota discovered when investigating failures in their state, both the decision to grant and the decision to revoke a license rarely take into account the actual operating faults of the centers. Substandard facilities are approved, on the basis of promises and hopes that conditions will be corrected and improved. All too often, however, improvement is not forthcoming. But then, "once a center is in operation with parents and children relying upon it, there is a great reluctance to revoke a license without alternative facilities available in which to place the children." In Minnesota, the League found, the state had no other power save licensing with which to enforce standards. And yet never once had it actually revoked one: This might have led to a "confrontation" with those operating and using them. Inevitably, then, as officials rush to use the centers as alternatives to welfare, they will grow even more lax about standards. A probationary license will become permanent, and then no one will want to put the center out of business. If the expansion of the day care center movement continues unabated, we will witness a further deterioration of these institutions from a point that is already far too low.

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Nor have the centers solved the problems of staffing. The issue is not primarily one of adequate funding, for the rapid turnover in staff that now plagues the centers, and the absence of any credentials among its personnel, reflect more than just a low salary scale. They also point to weaknesses in the founders' conceptions as to who would be running the institutions. Proponents insist that early childhood education experts will perform the task. It is the cognitive psychologist who is the contemporary counterpart of yesterday's "well-trained" social worker. Ostensibly skilled in teaching children to learn, he is to design the center's routine, and then train a group of workers to administer it. But in fact, there are few early childhood education experts around. Worse, there is no evidence that they actually do have the "answers" to early education. This is not a well-established field, with incontrovertible and tested principles that only need to be systematically learned and applied. Rather, the field is still too uncharted to serve as the basis of a day care system.

So the reality of staffing in centers today is, typically, that a group of women, without special training or even talent or aptitude, staff the centers, usually on a part-time basis. For many of them, it is a temporary commitment. At best, they run a genial custodial operation. At worst, they inflict the kinds of cruelties that amateurs in charge of such institutions are prone to commit.

Giving the poor a choice

There is a long and disheartening tradition in this country of reform rhetoric legitimating new institutions which, in the course of one generation, decline into custodial places, or worse. Some 150 years ago it was the almshouse that was going to bring habits of discipline and thrift to the poor, and thereby remove them from the poor-relief rolls; a later generation invested in clinics where social case work would teach the poor to "adjust" to their circumstances. In each instance, benevolence flowed from the top down, from experts in moral uplift or psychology or pedagogy to the poor and their children.

The day care center movement belongs to this tradition. As always, proponents are not at a loss for persuasive arguments. Isn't it good to get poor children out of rat-infested tenements for eight hours a day? What could be wrong with giving the mothers marketable skills? How can one object to intensive childhood education or hot meals and free medical check-ups? Are not all problems of power solved if the poor have a say in the administration

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of the day care centers? But as always, these considerations avoid the fundamental issues, which are economic and political. If we are going to spend 20 *billion* dollars for day care centers—the estimated cost of the Ribicoff proposal—and if most of this money goes to middle-class professionals, is that really the best way of helping poor families? And do we want to give government the right to coerce mothers into placing their children in day care centers so they can go to work?

Given the potentiality for abuse—indeed the already clear signs of it—in day care centers, it would seem the better part of wisdom to trust the poor rather than to trust the administrators or those who claim to speak for the poor. Why not allow the welfare mother to choose among alternatives? Rather than encourage the federal government to make appropriations directly or exclusively to day care centers, it would be more equitable and less risky to use the funds to establish a generous family assistance program. In this way, parents could exercise their own discretion. They could select this day care center or that one, or none at all. They could decide to leave their children under the supervision of someone else, or to forego job training in order to take care of them themselves. Critics are acutely sensitive to the dangers of abuse here—for the mother to remain idle or to spend her money foolishly. But they pass ever so lightly over the dangers of alternatives, such as founding institutions for child care that lack adequate supervision and personnel.

It remains all too true that getting federal money allotted to the poor is no easy matter. And many day care supporters, like Shirley Chisholm, urge the funding of centers not for their own sake but as a strategy for loosening purse strings in Washington on behalf of the poor. They believe that given the hyperbolic rhetoric surrounding day care, legislators might support the institutions handsomely, whereas they would not assist the needy directly. But the battle for appropriations is better fought directly. The prospect of coercion in a day care program more than outweighs its advantages as a fund-raising ploy.

In the end, the autonomy of decision making must be an essential part of any child care arrangement. First, because it is right and just that Americans control their own lives. Second, because it is the most important guarantor of success—only a voluntary commitment to job training or day care will allow it to succeed. If autonomy is not respected, we are sure in 10 years to be advocating, not day care as the right of every American child, but the organization of a Committee for the Rights of Children in Day Care Centers.

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Making wage controls work

ARNOLD R. WEBER

SINCE the end of World War II, almost every major nation in Western Europe has experimented with some form of direct government intervention to keep wage and price increases within the limits of economic stability. In the United States, the Kennedy-Johnson Administration took the first step in this direction by launching a program of "wage guidelines" in 1962-66. Although the effectiveness of this effort was (and is) still the subject of controversy, the Nixon Administration, on August 15, 1971, went the whole way and instituted a comprehensive program of direct controls on wages and prices.

Part of the continued attraction of controls to policy makers is a tacit assumption concerning the nature of controls: that they will be self-effectuating. Although the theoretical efficacy of economic controls is the focus of continuous examination, scant attention is given to those operational considerations that must be addressed to establish an effective program. This omission is not merely trivial or academic. To achieve success in an economic stabilization program, the controllers must display the sophistication of David Niven and the dexterity of Harpo Marx in pursuing the unwilling blonde. In controls as in courtship, innocent passion is more likely to lead to frustration than to consummation.

After more than a year of Phase I and Phase II, we have lost our

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innocence without enlarging our comprehension. What have we learned about the process of managing an economic stabilization program? The issues involved encompass both the economic impact and the political acceptability of any system of controls and should condition our expectations, if not our basic decisions, concerning future actions.

This discussion of the nature of controls is derived primarily from experience with the wage stabilization program. Because the Pay Board was originally established on a tripartite basis (with members representing business, labor, and the public), many of the issues were more plainly revealed—even though the level of discourse during this stage was seldom calculated to promote intellectual enrichment. Many of the following observations probably are also applicable to the proceedings on the price side, where technical analysis has sometimes masked the basic elements of policy.

Setting the standard

In developing a program of controls there is a basic choice between 1) laying down a tight general standard of permissible wage increases and applying a loose policy on exceptions and 2) establishing a relatively generous standard and adopting a stringent approach to exceptions. This platitudinous formulation of the issue should not obscure the important conceptual and strategic questions involved. No single number—three or six per cent—can insure economic efficiency or equity in a complex economy where each situation may incorporate a unique set of factors. Some workers may have realized sizable wage gains in the period immediately prior to the imposition of controls because of short-term scarcities or the power to exploit advantages over the employer in collective bargaining. The relative position of other employees may have deteriorated because long-term bargaining agreements forestalled wage adjustments, or because the demand for a particular occupation has declined temporarily.

Any system of wage controls must have sufficient flexibility to accommodate the needs created by these different situations. Complete latitude would be an administrative nightmare and would undermine the basic restraint that is inherent in any system of controls. Conversely, a rigid standard is sure to result in inequities for particular groups of workers or to impose inefficiency on individual employers who must adapt to changing conditions in the labor market.

The nature of the standard will, in large measure, determine the

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policy with respect to exceptions. Where there is a tight standard—defined by a large gap between a permissible increase and that which might be expected under current economic conditions—there must also be a flexible approach to exceptions. On the other hand, the promulgation of a generous standard means that most individual cases can be accommodated within this regulatory framework and that the goals of the controls program will have to be served by an austere approach to exceptions. As in baseball, if the pitching is tight, a porous defense will not be too damaging; where the pitcher is easy to hit, however, the defense should be impregnable, or defeat becomes inevitable.

During past exercises in wage stabilization in World War II and the Korean War, the controllers opted for the first approach. In contrast, the current foray into wage stabilization is founded on a loose standard with little latitude for upward deviations. During the Korean War the general standard limited wage gains to increases in the cost of living, but various categories of exceptions were established whereby particular groups of workers could exceed the standard by prodigious amounts. For example, special increments as high as three per cent were permitted to correct "intra-plant wage inequities." Such inequities usually are visible to any union leader or personnel manager who wants to see them. Similarly, the cost of fringe benefits was exempted from the calculation of permissible increases.

Under the present stabilization program, the general wage standard established by the Pay Board permits all workers to register wage increases up to 5.5 per cent without contriving any special justification. This standard was based on the summation of the expected annual increase in national productivity—as derived from long-run trends—and projected increases in the cost of living. This 5.5 per cent wage increment can be supplemented by an additional .7 per cent for certain fringe benefits such as pensions and health insurance. Moreover, total wages and fringe benefits have been permitted to increase by as much as 9.2 per cent in cases in which the workers' economic gains have lagged over the last three years. Only a rabid partisan would be able to view this standard as an instrument of oppression.

The policies adopted by the Pay Board have permitted few exceptions. No provision has been made for a special allowance for either intra-plant or inter-plant inequities. And all fringe benefits are subject to some limitation. Where bona fide inequities exist, it is expected that they will be ameliorated within the limits of the general pay standard. The standard may be breached in special situations, but

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only on a case-by-case basis at the discretion of the Board, not as a matter of right.

The difference between the approach taken in the past and the policies formulated by the Pay Board reflects the logic of economic circumstances and administrative requirements rather than conscious design. During the wartime experiences, the economy was characterized by full employment and a high level of aggregate demand for goods and services, so it was necessary to keep a tight rein on wages to avoid augmenting inflationary pressures. In addition, elaborate administrative machinery was constructed to deal with the myriad of exceptions that had to be made from the restrictive standard. Phase II, by comparison, was imposed on an economy marked by considerable underemployment of both labor and industrial plant. Thus wage increases linked to productivity and projected increases in the cost of living would not be expected to have an autonomous inflationary effect. In addition, the Pay Board has operated in a spartan administrative environment engendered by President Nixon's aversion to a "massive bureaucracy." The proliferation of exceptions would require a large staff, whereas the maintenance of a generous pay standard, largely administered by the parties themselves, has permitted the Pay Board to operate with a stripped down organization.

These differences in approach have had significant tactical and psychological implications. The tight-standard/permissive-exceptions approach creates a framework for bargaining rather than for decision making. With a relatively generous standard in effect, most unions and employers have been able to adjust wages without becoming entangled in the Board's procedures. At the same time, the penurious approach to exceptions has meant that the latitude for "making deals" above the standard has been severely constrained. This condition helps to explain the sense of repression that afflicted the labor members of the Board. In "the good old days," when a tight standard was in effect, numerous exceptions could be made without impairing the effectiveness of the program. Under the existing arrangements, however, the "deals" have been few and niggardly in the eyes of organized labor. Although four months were spent in a painful process of negotiating the policies of the Pay Board, the labor members never expressed the conviction—informally or otherwise—that the resultant standards had sufficient validity to be applied in any individual case that came before the full Board. Because the labor representatives viewed the Board's proceedings through a veil of nostalgia, there was an unbridgeable gap between the theory underlying the policy choice and the practical application of these policies in specific cases.

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Strikes and stabilization

The record of Phase II indicates that wage stabilization responsibilities can be carried out independently of a concern for averting strikes or settling disputes. Here again, the operational assumptions of the Pay Board stand in sharp contrast to the earlier applications of controls. In World War II, wage controls grew out of initial efforts at government intervention which had as their dominant objective the settlement of disputes. During the Korean War, the Wage Board put considerable emphasis on the maintenance of labor peace as well as economic stabilization. In both cases, the wage stabilization agencies had a strong representation of labor mediators and arbitrators.

The priority given to disputes settlement was appropriate to the circumstances that existed at that time. The nation was at war and the maximum utilization of labor resources was critical to the war effort. In addition, labor had taken a "no strike" pledge in World War II, and the stabilization agency had to adopt an activist role in the settlement of disputes in order to balance this relinquishment of economic power.

The present venture in controls has taken place in quite a different context. The requirements of the Vietnam conflict did not necessitate the widespread mobilization of the nation's resources, and significant slack existed in the economy. Consequently, the Pay Board has consciously eschewed any direct involvement in efforts at disputes settlement. Nor has it explicitly given weight to possible strike action resulting from its decisions.

As a body, the Board has adhered to the view that acceptance of the goal of disputes settlement would be inconsistent both with the economic premises underlying the stabilization program and with standards of equity. The conventional justification for wage and price controls is that powerful economic institutions engender inflationary pressures in the labor and product markets. To some extent, this power derives from the capacity to withhold services or production from the market. It would be self-defeating to accommodate power considerations in an administrative process that was designed to restore some balance, albeit temporary, to economic decision making. Acceptance of disputes settlement as a goal also would reward the strong while controlling the weak, who have only a limited capacity to inflict harm on the community. Such a policy would probably have the effect of altering the distribution of income in favor of those who are relatively well off and away from those who are at the bottom of the national wage structure.

The background of the original public members of the Pay Board

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(who continued to serve on the Board when it was reconstituted) reflected the narrower orientation of the current stabilization program. Only one of the five public members had any background as a labor mediator; the rest came from economics or the law. When organized labor assailed the public members as "amateurs" (which was one of their more charitable characterizations), this reference to a lack of bargaining skills reflected labor's belief that the new economic stabilization game should be played by the old rules.

Nevertheless, the Pay Board's activities do not appear to have contributed to the occurrence of labor conflict. None of the major decisions handed down by the Board has precipitated a strike. At the same time, the incidence of strikes, measured as a percentage of total man hours, has sharply declined since the stabilization program began on August 15, 1971. To be sure, this development has been aided by the fact that 1972 has been a light year on the national bargaining calendar, but the level of industrial conflict is also lower than it has been in comparable years in the normal three-year bargaining cycle.

The fact that the Pay Board has adopted a policy of economic primacy does not mean that all of its decisions have exhibited an Olympian detachment. Undoubtedly, the Board's decisions in the coal and railroad cases—where agreements far in excess of the general pay standard were approved—reflected management's anxiety over the effect on industrial production of possible strikes in these essential industries. During the public hearing on the coal case, W. A. Boyle, President of the United Mine Workers, dryly noted that "you can't mine coal with bayonets." The proletarian romanticism of this statement did not detract from its validity, and the contract was ultimately approved by a 10 to five margin, with the majority comprised of the labor and management representatives.

Similarly, it was difficult to consider the longshore cases in purely economic terms. Prolonged strikes on both the West and East Coasts had had a significant impact on the economy, rippling back through the channels of distribution to the farmer and manufacturer. The prospect that a national maritime shutdown might be precipitated by a cut in the settlements that had been reached between the parties could not be readily ignored. The implications of these cases became more portentous because of their timing. The East Coast settlement was considered by the Board immediately before President Nixon's trip to Moscow. One of the President's bargaining chips was the possibility of expanding trade between the United States and the USSR. This prospect had been enhanced by the recent agreement of the International Longshoremen's Association to load American wheat on

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foreign ships for movement to the Soviet Union. A cut back in the settlement could have destabilized labor-management relations in the industry and jeopardized the agreement. While the Board discussed this exigency, it ultimately accepted a less global definition of its responsibilities. In the absence of any statement from the Administration that a reduction of the wage agreement could cast a shadow over the President's trip to Moscow, the Board made its decisions on the narrow merits of the case.

The management game

The unruly history of the Pay Board confirms the judgment that management's ability to represent its *class* interests effectively is limited because of its indulgence toward *specific* interests. Most executives of large firms have actively supported wage and price controls. Indeed, pressure from the business community was one of the important factors that precipitated the Administration's *volte face* in economic policy. Underlying the business community's ideological commitment was its belief that the current inflation was caused by cost-push factors. However, when it came to particular cases, management revealed an incapacity to place class interests above specific interests. Although the broad interests of management might be served by restraining wage increases, most executives have demonstrated an incorrigible sensitivity to the "uniqueness" of specific cases and have succumbed to anxieties that Pay Board decisions might result in an impairment of a bargaining relationship, a strike, or a loss of business.

This schizophrenia strengthened labor's hands within the Pay Board. The labor representatives publicly brooded over "an unholy alliance" between the business and public members, but the more relevant danger was the short-term coalitions of convenience which developed between business and labor in particular cases. The business representatives voted with the public members in the affirmation of *general policies*, but they lined up with the labor members in the first major cases considered by the Board. In every case that has come before the full Board, the management involved has made a plea for an exception with varying degrees of passion or resignation.

For the labor members, there was a perfect identity of ideology, intellect, and interests in conducting their business on the Board. The labor votes were always completely predictable, and labor's tactical approach was to attract a brace of "doves" among the business or public members on any individual case. On one occasion, two of the

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labor representatives indicated a willingness to accept a modification of a major contract by abstaining from voting on the case. But the pressures for solidarity were sufficiently strong so that ultimately these members voted as a bloc with their brethren.

Protecting "the little fellow"

Although each segment of the Pay Board has been outspoken in expressing a fervent concern for "the little fellow," the administration of a stabilization program inevitably works to the advantage of "the big fellow" and to the detriment of "the little fellow." This bias is inherent in any regulatory system. While the Pay Board staff views its regulations as a picture of simplicity, for most union officers and businessmen the pronouncements gushing from the Federal Register have all the clarity of instructions for the assembly of Japanese sewing machines.

A regulatory system is, of course, a legal system. This means that in order to exploit fully the latitude permitted by the regulations it is desirable, if not necessary, to retain counsel and to move with some sophistication through the labyrinth that is created by any regulatory program. Since few small employers or unions can afford top Washington law firms, they are left to circumnavigate the agency's procedures with a pocket compass. Moreover, the composition of the Pay Board did not create a structure in which the problems of "the little fellow" were likely to be aggressively promoted or understood. The business representatives came from companies like General Electric, Continental Oil, and the Southern Pacific Railroad—hardly exemplars of the petty bourgeoisie. On the labor side, the representatives were top officials of the United Automobile Workers, the United Steel Workers, the International Association of Machinists, and the Teamsters—the behemoths of the American labor movement. It is significant to note that while most employers with less than 1,000 employees have limited wage increases to 5.5 per cent or less, workers in unionized situations with more than 5,000 employees have registered a weighted average wage gain of 8.4 per cent in the first nine months of the program. In the large, unionized situations the parties may be expected to employ maximum sophistication—and leverage—in responding to the system of controls.

The fact that the early representation on the Board and the inherent bias of regulatory procedures has worked to the advantage of "the big fellow" means that economic controls cannot be used to redress the basic power balance that exists in the market. This is not

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to argue that the stabilization program should embrace this objective; nonetheless, many advocates of controls have given it a prominent place on their hidden agenda. Yet if controls have any effect on the relative distribution of bargaining power in the labor market, it is probably in favor of the large agglomerations. This contention underlies the fact that periods of controls in the past have been associated with significant increases in union membership. What economic planners may find difficult to accept is easily understood by the individual worker, who recognizes that in a system of administrative wage setting clout is as important as compassion.

Unwittingly, the Pay Board did take a significant step to protect "the little fellow" from egregious mistreatment at its hands. Because the Board set a loose standard, the need to finesse its administrative procedures to gain special treatment was sharply diminished. If one of the consequences of the Pay Board's approach to economic controls has been to minimize the opportunity to use the program as an arena for further bargaining, then a corollary is that large numbers of employees and employers also have been afforded the discretion to make significant economic adjustments without having to buy a hand in the only game in town.

Economics and equity

While wage controls have been instituted to achieve technically defined economic goals, the actual process of applying these controls becomes as much an exercise in equity as in economics. One of the virtues of the free market and free collective bargaining is that these processes of decision making are essentially amoral in nature. When a non-union employer sets a wage he does not review the writings of St. Thomas Aquinas. Rather, he examines the applicable market rate, the available labor supply, the need for any special skills in the firm, and related factors. Similarly, when union and management representatives sit down at the bargaining table, they may engage in homiletics, but they know that the agreement will be determined by the prevailing bargaining pattern, the economic status of the firm, labor market conditions, and the capacity of each party to inflict harm on the other. Some observers may rue the absence of loftier principles, but in the day-to-day activities of the labor market wage decisions are dominated by technical or tactical considerations.

With the launching of the stabilization program, powerful new equitable considerations have been introduced. First of all, the administrative procedures inherent in the system of controls make ex-

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plicit and expand the framework for equitable comparisons. Normally, a wage agreement reached in the railroad industry would have virtually no effect on wage determination in the aerospace industry. But under the system of controls, where both decisions are made by the same group of frail mortals, the relationship between the decisions in the two sectors has to be kept clearly in mind.

Moreover, each individual case may have its own compelling considerations of equity. In the longshore cases, for example, increases significantly above the general wage standard were approved because the unions had agreed to changes that would significantly increase productivity and reduce costs. In the coal industry, even the public members voted for the approval of a 12 per cent wage increase, a substantial part of which was necessary to replenish a pension fund on the edge of insolvency. Because the coal industry has been marked by sharply declining employment, a work force of 130,000 supports a fund paying benefits to 500,000 recipients. The most relentless controller would be reluctant to support a decision denying retirement and health benefits to incapacitated miners, widows, and orphans.

Unions essentially are engines of equity operating in an economic environment, and they are particularly sensitive to considerations of "fairness." Dry statements of labor market conditions, productivity trends, and profitability will provoke eloquent arguments from union leaders about "social justice." These pleas are not frivolous; they serve to define the boundaries of the national consensus that must be preserved to sustain any system of economic controls. Even though the bulk of organized labor formally withdrew from participation in the stabilization program, the decisions of the Board must respect the limits of "fairness" in order to win the acquiescence of broad sectors of the community in its decisions.

The intrusion of equitable considerations into the stabilization program means that any effort at controls inevitably becomes politicized. The process of politicization often acquires a partisan coloration—i.e., Republicans extol the sagacity and effectiveness of the program while Democrats excoriate the Administration for its ineptness and favoritism. But in a more basic sense, the system of controls becomes intrinsically "political" to the degree that it must accept the relevance of equitable considerations. As "efficiency" is the *sine qua non* of economics, so "equity" is the cutting edge of politics. While controversies over the economics of the stabilization program are complex and subject to normal obfuscation, they are resolvable in the mechanical sense. In contrast, disputes over "equity" or "fair-

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ness" almost never lend themselves to technical resolution; for each party's view of what is "fair" is the product of ideology, interests, and a range of other subjective factors. The difficulty, if not the impossibility, of administering controls in a way that it is viewed as "fair" by all the parties affected is the most serious problem facing any stabilization program.

A reasonable assessment of the policies and decisions of the Pay Board would support the conclusion that labor was not victimized during the period of its active participation in the program. Of the 54 votes conducted by the Pay Board during labor's tenure on the Board, the labor members voted with the majority 36 times. The contention that many of these votes were *pro forma* or were designed to stave off more vindictive decisions is discounted by the fact that 27 of the votes were unanimous and thus labor's votes were unnecessary to avoid inequity. In addition, the Board voted to sustain labor's petitions in five of the eight cases that it considered before the labor members' mass departure.

Nonetheless, organized labor's sense of grievance no doubt was real. When it became apparent that little public sympathy could be elicited about the reduction in the West Coast longshoremen's wage increases (leaving the workers with a total hourly compensation of $8.39), the labor assault on the program shifted to a wider framework of equity. With well-televised indignation, the AFL-CIO bitterly complained that while wages were being effectively controlled, prices and profits continued to rise, levying an unfair burden on the worker.

What price controls?

A program of wage controls is not self-liquidating. The imposition of controls is not like a surgical exercise in which some benign tumor is excised from the body. Instead, the process of regulation itself creates a set of problems that will have a significant impact on future events and that may require subsequent regulatory measures. Thus the policies adopted during the wage-price freeze created great contention over the payment of retroactive wage increases and over the legal status of increases that had been negotiated before August 15, 1971, and were scheduled to take effect during Phase II.

The history of wage controls throughout the Western world indicates that once they are removed a "wage explosion" takes place. It is clear that the current stabilization effort has created a set of incentives and constraints that have caused labor and management to

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change their normal modes of behavior. Although the West Coast longshoremen's union reluctantly accepted the Pay Board's ruling, it inserted a clause in its contract providing that wage negotiations could be reopened within 24 hours after controls have been eliminated. Even the most myopic observer can recognize that the union will attempt to use this clause to regain the increases that were denied by the Pay Board. Several other unions have adopted the same tactic. On a more general level, the negotiation of multi-year contracts, normally three years in duration, has been one of the major achievements of the American system of industrial relations. In view of the uncertainties introduced by controls, some unions, especially those in the construction industry, have been shifting to shorter-term contracts. This retrogressive development holds the prospect of greater instability and the reinstitution of what was once called "the guaranteed annual argument."

All of this implies that it will be extremely difficult to terminate the wage stabilization program with discrete finality. Instead, some form of government intervention may be necessary to "wind down" the level of controls in order to avoid the shock to normal institutional practices. In the absence of some transitional device, such as a public review board to intervene selectively in major wage decisions, efforts to deal with these problems "cold turkey" will result in both private and public costs. And the longer that controls are maintained, the more severe will be the problems of normalization.

This recitation of the perils of the Pay Board does not mean that economic controls are either virtuous or perfidious. Such normative judgments are easy to make but difficult to defend. The basic lessons of the current stabilization program are more complex and less conclusive. After one year of experience, it is clear that controls are not a form of economic alchemy that will produce a golden stability by incantation. The technical problems alone are formidable, and the margin for error is especially slim during peacetime, when the national consensus necessary to support a system of controls is fragile or transient. For every editorial urging the Pay Board and Price Commission to "be tough," there is a counter-chorus crying, "be fair."

The record of the Pay Board also indicates that there may be significant costs involved in the imposition of economic controls. The most obvious are the diminution of freedom and possible loss of efficiency in the operation of the market system. This is an old issue in the conduct of economic affairs, but obviously a substantial segment of the nation is willing to incur these costs for the prospective benefits of stable growth. But other costs must also be weighed, such as the

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effect of the program on the distribution of income; then, too, there is the cost of interjecting another divisive issue into American political life. Moreover, controls may change the nature of the institutions whose behavior they seek to stabilize. It is yet to be determined, for example, whether the legacy of wage controls will be new approaches to responsible collective bargaining or a rash of redemptive strikes and a hardening of union-management attitudes.

Economists like to divert themselves with extended arguments over whether controls can or cannot "work." This issue is largely irrelevant. The real issue is whether society is willing to assume the direct —and indirect—costs that are necessary to make them work. Controls can help in the quest for economic stability, but they are neither self-effectuating nor free. And they may permanently alter the situation they were designed temporarily to control.

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Woodlawn: the zone of destruction

WINSTON MOORE, CHARLES P. LIVERMORE, & GEORGE F. GALLAND, JR.

It was a moderately distinguished group that took a walk one morning a year ago through the devastated blocks of Chicago's Woodlawn neighborhood—Senator Charles Percy, U.S. Representative Abner Mikva, Assistant Secretary of Housing and Urban Development Floyd Hyde, and local HUD chief John Waner. They were investigating a local newspaper's suggestion that the black South Side community was being systematically burned to the ground by a conspiracy of real estate operators and public officials. Reporters waited, pencils in hand, for the visitors' reactions to the seemingly endless line of abandoned apartment houses and rubble-strewn vacant lots. But there seemed to be little that anyone could say. Mr. Waner reflected on what he saw as a collapse of civic pride: "What in hell happened to the 'I Will' Chicago spirit?" Assistant Secretary Hyde was reminded of home: "You could put those buildings in the Georgetown section of Washington and they'd look good." And Senator Percy summed up the general mood of astonishment and bewilderment: "Where does it all end?"

America has always had slums. But for the most part they have been "good" slums, as slums go: They stayed put for a while, they had a certain stability, sometimes they endured, and if they changed, they did so slowly. Woodlawn, by contrast, was a terrible slum: *It*

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did not last. Large parts of it simply disappeared, and towards the end it disintegrated with extraordinary speed. In other neighborhoods in other cities across the nation, other slums are increasingly doing just what Woodlawn did—disappearing. The problem with this process is that it creates human pain and tragedy on an immense scale. That much is understood by practically everybody now, thanks to the attention paid to Woodlawn by the media. It isn't pretty to watch whole communities self-destruct in the heart of the cities of the world's richest country, nor is it good for America's self-respect to feel that it has a case of incurable cancer. But just to feel bad about it, or deplore it, is not quite enough. We also need to take a serious, unsentimental look at the reasons for the collapse of neighborhoods like Woodlawn, for until we understand this process, we are not going to be able to abate the suffering and other problems it entails—and we may in fact unwittingly exacerbate them.

The cycle of abandonment

Some of the larger reasons for what happened in Woodlawn are clear enough. First, we are at the end of an historic cycle of displacement. The huge European migrations of the 19th and early 20th centuries are now over, and the 20th-century migrations of blacks from the rural South to the urban North have slowed down. The succession of newly arrived population groups which caused neighborhoods to shift from older to newer immigrants is at an end, and in older neighborhoods like Woodlawn there is no new group on the horizon to succeed the blacks. These older black communities are therefore losing population and have housing surpluses. They are largely populated by a destructive residual underclass. This underclass is not something brought into the city by rural-to-urban migration or an indigenous black culture; it is instead largely the product of urban welfare policies, which institutionalize poverty, stifle upward mobility, and discourage stable family formation for large numbers of blacks. As a result, the environment of these older neighborhoods becomes so dangerous and the management of housing so risky that no new population will move into them, and no one will invest in them except as part of a large-scale development which has the capacity to *exclude* the underclass.

Second, there is the arithmetic of housing. Real estate is a business. You can make money on used housing if you can sell it or rent it for a profit. If you can't do either, there is no reason to hold it. As long as middle-class and working-class families lived in neighborhoods

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like Woodlawn, it was possible to charge rents sufficient to make a profit on rental housing. But along with their white counterparts, black middle-class families have been leaving Woodlawn for newer communities where they can own their own homes. Those who couldn't afford to become homeowners have had to move out as the growth of lower-class crime has made the neighborhood unsafe to live in. That has left only the very poor, who cannot move. There is money to be made by developers and builders of new subsidized housing. But there is no money to be made from used private housing for the black lower class, and neither the black or white middle class will reclaim it. So it is abandoned—thereby giving rise to the spectacle that shocked Senator Percy and his colleagues.

Third, much of the housing occupied by the black families who came in the big migrations during and after the World Wars was already old and worn out when they arrived. Old housing may last forever in rural New England towns and in stable communities, but it wears out rapidly when subjected to abuse or even hard use by poor people with large families.

Fourth, personal security in neighborhoods like Woodlawn has broken down so completely that few families with any choice want to live there. Traditional police techniques for maintaining order depend on assistance from ordinary community and family institutions which most neighborhoods can take for granted. When a neighborhood like Woodlawn reaches the end of its historical line of succession, these institutions no longer function. Being the only enforcer of neighborhood security is a new role for local government, one which it has not yet learned to perform effectively. We haven't yet found ways to deal with the problems of kids in these neighborhoods who get into trouble, or to get adequate school supervision over kids who get none at home, or to provide proper law enforcement when all fear to testify.

As conditions get worse, everybody runs who can. All that is left is a population no landlord wants, a dangerous underclass spawned by poverty, joblessness, misguided welfare policies, and ineffectual corrections and educational programs. In the final stage, the kids and young adults of this underclass cannibalize their own neighborhoods, stripping vacant apartments of anything of value, doing $30,000 worth of damage to a house to get a few pounds of brass fittings and copper wiring to sell to junk dealers for a few pennies.

These, as we said, are the general outlines of the abandonment process, which are well known. But why it happens in any particular neighborhood, and what precisely goes on there, is not so well

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understood. Woodlawn provides a vivid illustration of one particularly grave case of the general phenomenon. As we shall see, it does not happen altogether automatically, and both government and private institutions play a significant role.

A portrait of an old neighborhood

Woodlawn is a neighborhood of less than two square miles near Lake Michigan, immediately south of the University of Chicago and eight miles south of Chicago's Loop. Population began to accumulate in the Woodlawn area after 1880. Never notably fashionable, the neighborhood developed a large white transient population in boarding houses and apartment hotels built during the first decades of this century. Its major foreign groups were Irish, German, and English, but apparently it never developed the pronounced "ethnicity" characteristic of some other Chicago neighborhoods. Its racial transition covered 40 years. Blacks in Woodlawn, according to the U.S. Census, increased from 13 per cent of the total population in 1930 to 39 per cent in 1950. During the 1950's the transition accelerated, and by 1960 only 8,450 whites were left, most of them living on the southern fringe of the University of Chicago.

As Woodlawn went black, its total population rose—from about 66,000 in 1930 to some 81,000 in 1960. The age composition also changed: In 1930 only 4.8 per cent of Woodlawn's inhabitants were under five, and only 21.7 per cent were under 20, but by 1960, 13.6 per cent were under five and 34.2 per cent were under 20. As Woodlawn's housing grew older, it grew more crowded. The total number of dwelling units rose from 27,624 in 1950 to 29,616 in 1960, an increase due to subdivision (mostly illegal) of existing structures, since new construction came to a halt in Woodlawn during this period. Of the total number of dwelling units, only 8.8 per cent were owner-occupied in 1960 (the city-wide figure was 32.7 per cent). Particularly significant was the high concentration of large apartment houses: 14,359 of the 29,616 total dwelling units were in structures with 10 or more dwelling units apiece. This concentration of large, generally absentee-owned rental buildings was matched elsewhere in Chicago only by the wealthy new lakefront areas. This is a critical fact in understanding neighborhood decay, since abandonment is usually a problem of rental housing, not privately owned homes.

In 1960, at the beginning of Woodlawn's disastrous decade, the community was a crowded, largely rental-occupied black neighbor-

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hood, most of whose housing was at least 40 years old. An awesome collapse followed. During the 1960's Woodlawn's population declined from 81,000 to 52,000, a drop of 36 per cent, and it is still dropping; the core of Woodlawn, between Cottage Grove Avenue and Jackson Park, lost 41 per cent of its population. These losses exceeded those suffered by any other Chicago community. Between 1965 and 1971, the city demolished over 400 Woodlawn buildings. Lately the city has been demolishing at a rate of 500 dwelling units a year in Woodlawn, and currently has a demolition backlog there of over 1500.

How the housing market collapsed

Numbers like these are evidence of a complete collapse in the housing market. The dismal arithmetic of Woodlawn real estate can be looked at from either the supply side or the demand side. Ironically, what originally began to deflate demand for Woodlawn's rental housing was the expansion in housing opportunities for black families in other areas of Chicago. During the 1950's and 1960's, large numbers of white families left the central city and bought homes in the suburbs. The same pattern has been taking place, on a smaller scale, within the black community. In 1940 the black population of Chicago was 282,244, three quarters of whom lived in just three neighborhoods on the Near South Side—Douglas, Grand Boulevard, and Washington Park—which were Chicago's only black-majority neighborhoods. By 1960 Chicago's black population had grown threefold, to 812,637, and 15 community areas were now predominantly black. In 1940, only about five per cent of black-occupied dwelling units were owner-occupied; by 1960, however, the proportion had risen dramatically to almost 15 per cent. The growing black population was moving into white neighborhoods where single-family houses predominated, and for the first time developers were putting up new housing to sell to blacks, especially on the far South Side.

The blacks are thus following the pattern set by many white immigrant groups, who first established themselves in older rental housing and then gradually moved outward toward homeownership as they moved up the economic ladder—a process described by the "trickle-down" theory of the housing market. But the difference between such previous groups and the blacks is that no one is replacing the blacks as they move out of older rental neighborhoods like Woodlawn. There is no group left for the housing to trickle down to, and middle-class blacks do not have to continue living in it. Today a

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middle-class black family is not forced to rent housing in older "renter" neighborhoods such as Woodlawn. It can generally buy a house in one of the changing neighborhoods, or in one of the new developments aimed at blacks. Committees seeking to help black families enter white neighborhoods report more housing opportunities than takers. If a black middle-income family would rather rent, it may be able to do so in a new high-rise development rather than in an old Woodlawn walk-up. For the black professional family, to integrate or not to integrate is a live option.

The result of all this is that demand for the kind of housing that predominates in Woodlawn—large, rental, walk-up apartment buildings—is increasingly concentrated at the low-income end of the spectrum. Except in a few special situations, this particular kind of housing seems to have lost favor with prevailing American tastes. Areas with large concentrations of single people or newly-formed families still seem able to support large concentrations of older rental walk-up housing, but by and large this kind of housing is for people who have the least choice. The "core" of demand in the Woodlawn housing market—demand by middle-class families—has disappeared. Thus even in the absence of criminal behavior and the other disincentives of a deteriorating community, Woodlawn's housing would have held decreasing attraction for the middle class, black or white.

Woodlawn's rental housing could probably have continued to serve the many working families who had enough income to pay rents required to make the housing profitable, but who were unable to become homeowners. Such families form the nucleus of many of Chicago's white working-class communities, and rental housing in these areas still seems viable. But when the explosion of lower-class crime in Woodlawn forced out even those who could not afford to improve their housing situation, the bottom dropped out of the demand for Woodlawn's housing. In short, changing tastes and opportunities caused those to leave who could afford to move up the housing ladder; criminal terror pushed out the rest.

What made this two-pronged collapse of demand so disastrous was that meanwhile the cost to property owners of supplying rental housing was steadily rising, and rising much faster than the incomes of those who rent it. In Chicago as a whole, the average increase in rents from 1950 to 1960 was 100 per cent, while the average increase in income was only 65 per cent. For Woodlawn, the average increase in rents was 71 per cent, but the average increase in income was only 38 per cent. Preliminary data from the 1970 census indicate that during the 1960's rents in Woodlawn rose by about 25 per cent, but

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that the incomes of Woodlawn's renter population increased by somewhat less.

The continuing increases in rent cannot be explained adequately by attributing them to rising demand for rental housing, for the total population of Chicago has declined over these decades, and there has been an increase in homeownership relative to rental occupancy. What does explain these changes is that, during the 1960's, the concept of strict code enforcement took root as part of an effort to cut down illegal conversion and to force landlords to maintain property. Property taxes went up, and so did maintenance and fuel costs. As the relative ability to pay of Woodlawn's residents declined, the cost of providing them with housing increased. The amount of maintenance required to keep buildings in decent condition rose rapidly as overcrowding and vandalism spread. For a landlord faced with such a situation, and no visible market for his property, the question is not *whether* he will abandon his building; it is only *when*. One Illinois State Senator understated it nicely: "There might be something in the economics of the situation that destroys housing."

Perhaps the most concise description of this cycle comes from a group of predominantly black-run banks and savings and loan associations. They hold mortgages on properties in Woodlawn and similar neighborhoods. In a brief document pleading for government assistance to housing, they wrote:

> The mortgagee usually starts with an average mortgage of 70 per cent of the appraised value of the building, for an average term of 15 years. When the mortgage is made the building is basically sound and in good condition.
>
> However, because these buildings are old they deteriorate at a very rapid rate. Added to this is the problem of rising expenses such as taxes, fuel, insurance and maintenance. The only expense that remains constant is the mortgage note. These expenses rise at a greater rate than the landlord can raise rents. The result is that, on the average, in five years a building that made a profit at the beginning of the mortgage term no longer makes a profit.
>
> The owner in order to make ends meet cuts back on the only expenses that he has some control over—maintenance. This reduction in maintenance is at a period in the building's economic life when more and not less maintenance is required just to retard normal deterioration—old things deteriorate at a faster rate than new things. The value of the building is, of course, diminishing during this time.
>
> The mortgage balance during this five-year period has not been reduced by a third, because a mortgage balance reduces more slowly at the beginning of the mortgage term. With the value of the building

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diminishing faster than the mortgage balance is reducing, what began as a 70 per cent mortgage becomes a 90-100 per cent mortgage.

The regulations governing the mortgagee would not allow further advances or increases of the loan when these conditions occur, along with the fact that it would not be good business to send good money after bad. Needless to say, generally other avenues of financing repairs and rehabilitation are not available. With no repairs and rehabilitation, demolition is the ultimate result.

Enter the Blackstone Rangers

Woodlawn is a perfect inner-city case study not only for the collapse of its housing market, but also for the cycle of crime that combined with the arithmetic of real estate to wipe almost the entire community off the map. Woodlawn's crime problem became nationally famous. For years its street gangs were surrounded by an aura of romanticism and ideological controversy that made it difficult for many well-meaning people to see what they really represented. But now that the drama is drawing to a close in a heap of rubble, it is possible to pick out the stages that crime in Woodlawn went through.

The cycle began with a rapid build-up of poor blacks—many of them part of large families headed by dependent mothers—in Woodlawn's dense apartment clusters. Disorganized families who cannot get into public housing have few choices. Homeowners in the black community avoid renting to them because of their destructiveness. They must seek out large, absentee-owned apartment buildings that are increasingly shunned by more prosperous families. Because of its abundance of such buildings, Woodlawn was practically guaranteed a denser concentration of disorganized families than any other black area of Chicago. While many Chicago neighborhoods were "going black" during this period, it was the older, heavily built-up neighborhoods like Woodlawn that received most of the underclass. Gangs formed, as they always have in poor neighborhoods. But until about 1963 or 1964, they were small and they seemed little different from the traditional street gangs fighting over their turf.

The "baddest" of the gangs in Woodlawn was the Blackstone Rangers (named after Blackstone Avenue, which ran through the area), later to become the Black P. Stone Rangers and finally the Black Peace Stone Nation, in an evolution from one small gang to a syndicate of gangs. In the early 1960's they were a group of 50 to 100 kids. Most of them had police records; they had been kicked out of the Boys Clubs because of a disagreement over a juke box, and kicked out of a Lutheran church for writing bad words on the

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walls of the toilets. Finally a worker from the Chicago Commission on Youth Welfare found them a meeting place in the First Presbyterian Church, a massive remnant of the days of Presbyterian affluence in the area.

Its white pastor, Reverend John Fry, had won a national reputation for social consciousness. He was anxious in his new assignment to perform a significant ministry with Woodlawn's youth. He could not have hoped to succeed without considerable help—but help was on the way. After the riots of 1965 and 1966, national attention focused on the "ghetto." In Chicago, Woodlawn was a particularly convenient and interesting ghetto. It was already well known because of Saul Alinsky and The Woodlawn Organization (TWO). For the University of Chicago and its affiliated theological schools, Woodlawn was an irresistible laboratory for testing various social theories. From an early date, gangs in Woodlawn received organizational help from detached workers, from students, and later from antipoverty agencies. Reverend Fry wrote that while the Rangers had only 200 members in the fall of 1965, over 1500 showed up at a meeting in mid-1966. As he later told the United States Senate, "The success of this organizational work was—let us tend toward a modest word—incredible."

The hopeful attitude that led many diverse groups to give organizational and financial help to Woodlawn gangs shows up in a TWO request to the federal government for a grant of nearly $1,000,000 to provide job training to gang members. TWO summarized its project expectations as follows:

A. That the social structure of two street gang groups will provide a natural community-peer grouping that can play a strong positive role in self-help community unity and development.
B. That the natural structure of two street gangs provides an excellent vehicle to guide and develop skills and attitudes beneficial to the young adults of Woodlawn, the local community, and the larger community of Chicago.
C. That the natural structure of two street gangs is a logical mechanism to shift attitudes of disaffiliation to productive adult concern of employment, community development and upward mobility.

The program which ensued collapsed after a year and was investigated by the United States Senate amid widespread evidence of fraud by the leaders of the gangs.

This was only the largest of a number of official and unofficial programs that centered on the gangs. Private contributions also played a prominent role. A typical example was Charles E. Merrill, Jr., who gave $23,000 to go toward "black capitalism" organized by

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the Black P. Stone Nation. By demonstrating the profitableness of large-scale organization, such contributions of money and organizational help shifted the attention of gang leaders from petty neighborhood fights to large-scale violence. The gangs fascinated people concerned with social problems. Senator Jacob Javits made a trip to Chicago and invited them to dine with him. They accepted—and kept their hats on during the meal in the hotel dining room. During this same period, Martin Luther King, launching the Southern Christian Leadership Conference's (SCLC) open housing campaign in Chicago, was encouraged to enlist the Woodlawn gangs in his cause. Already having found a sympathetic ear in some of the socially conscious newspaper reporters, the Blackstone Rangers now had the mantle of racial heroes thrust upon them.

Such flattery was fuel to the gang leaders' fire. If intimidation through violence on a small scale produced a trickle of affluence, on a larger scale it could produce a flood. Their power depended on their numbers; their numbers depended on vigorous recruitment; recruitment depended on terror; and so terror became the name of the game. It was simple—join the Rangers or be shot.

The terror

"Stones run it" was their claim—and as far as the streets were concerned, they did. To prevent shooting in the local high school, gang members were made hall monitors. Over one thousand students fled Hyde Park High School in a single year, many of whom apparently went to live with relatives in the South. There was hardly a doorway in any rented building in Woodlawn that was not painted with the initials of the gang. Due to the recent marketing of a spray-paint can, scores of buildings could be decorated in a very short time. The walls of schools, churches, houses, and even police stations helped carry the message. Those who couldn't read were beaten up. Murder and arson eliminated resistance. By the time Dr. King's organization had decided that the aims of the gangs were incompatible with the SCLC program, the gangs no longer needed Dr. King. The Presbyterians stayed with them, other church foundations defended them, rich scions hired them lawyers, and the federal government sent special emissaries to help them prepare applications for federal funds.

This expansionary phase of gang activity and underclass crime carried the death blow to a community already beset by problems and whose increasingly unprofitable rental housing was on the verge

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of abandonment. Even without underclass gang terror the working and middle classes had a tendency to leave Woodlawn's older rental housing for newer neighborhoods. But the terror of the gangs finally put to flight even those who couldn't afford anything better. That destroyed whatever economic viability was left in Woodlawn's rental housing. Not everybody left, of course; to this day there remain in Woodlawn many families that survived the chaos, homeowners who couldn't sell their houses or who refused to flee. These solid citizens, with their churches and block clubs, made up the core of TWO when it started, and they still do.

As Woodlawn's rental housing fell into ruins, gang activity shifted to other neighborhoods. When E. Duke McNeil, President of TWO, was asked this year whether gangs were behind the Woodlawn fires he replied, "What gangs? Most of the leaders are in jail and the rest have moved away, like 25,000 or 30,000 others." Time began to catch up with the Woodlawn gang leaders. Criminal convictions have put many of them in jail.

The devastation of Woodlawn

A wave of arson and vandalism completed the community's criminal cycle. Vandalism is profitable to various people for various reasons. It has been speculated, although not proved, that owners sometime hire arsonists to gut their buildings. Destruction by fire is the clearest way under the income tax law to claim a tax loss. One unidentified real estate manager told a Chicago *Daily News* reporter, "It is known that if you want a fire to occur, you can arrange to have it done. And if you can't make money on a building, you sure as hell can collect insurance on it by burning it down. I know that if I wanted a fire, all I'd have to do is make a phone call—let it out—saying I wanted a fire. The word circulates and I would be contacted. The going price is $600."

But owner-sponsored arson probably accounts for only a small part of the fires. In abandoned or semi-abandoned buildings, thieves strip the fixtures, beginning with major items like water heaters and continuing down to the plumbing and wiring. Setting fires makes it easier for vandals to get at fixtures buried in the building structure and to push out the remaining tenants so that the whole building can be stripped down. And many fires probably represent arson just for the hell of it.

The theory that a real estate developers' conspiracy is responsible for the final collapse of Woodlawn has such an attraction that even

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experts who don't believe it sometimes espouse it, apparently just to see whether they can get away with it. Leon Despres, the independent Alderman from nearby Hyde Park whose ward includes part of Woodlawn, said suggestively: "If there were a conspiracy this is just the way it would happen." And in a December 1971 speech to the Hyde Park-Kenwood Community Conference, urbanologist Pierre deVise reviewed newspaper suggestions of how conspirators have buildings burned down. He continued in the following fashion: "Vacant parcels are then assembled by the city and either held or sold to parties known only to the Southeast Chicago Commission, the University of Chicago's own version of the CIA." This remark provoked an immediate uproar, and a few weeks later deVise backtracked: Only the obtuse, he implied, could have failed to realize that he was being facetious. To his credit, he then delivered some straight talk: "Right now there does not seem to be a market for any kind of housing in central Woodlawn. That, I suggest, is the main reason for the Woodlawn blitz."

The trouble with the conspiracy theory is not that it is implausible, but that the forces dismantling Woodlawn need no help from conspirators. Conceivably a few could be found, but the Woodlawn process does not depend on them. This is becoming evident as neighborhoods in less desirable locations have begun to go through the same process. No one has thought of the west side of Chicago as a real estate developer's dream; yet when fires and abandonment began there, the Northwest Community Organization passed a resolution charging that "a conspiracy of city officials, real estate speculators, mortgage houses, and others is responsible for the fires in order to scare us so they can tear down our neighborhood to build a high-rise for people in the suburbs." Building abandonment and fires are beginning to plague other black neighborhoods far inland, which hold no self-evident charms for speculators. One such neighborhood, Lawndale, has probably suffered more heavily than Woodlawn, according to a recent federally sponsored study of abandoned buildings in Chicago and elsewhere.

Should something be done?

What can be done to prevent what happened in Woodlawn from happening elsewhere? That question raises another question: *Should* anything be done? There are obvious advantages to letting the Woodlawn process go on. After all, as long as there is any housing left in the city, there will always be somewhere for the

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poor to live. As the more affluent classes, black and white, flee from the advancing underclass, the black poor will take over the housing they leave behind—and this, in turn, may well mean that the supply of housing for the black poor will get better. They have finally worn out much of the oldest housing; the neighborhoods they will inherit from here on will be newer.

Furthermore, while there are losses involved, they are borne largely by private investors, often other blacks, who lose their equity and by lenders who hold mortgages. This doesn't show up as a direct loss to the average taxpayer. And losses in the city may be compensated by gains elsewhere. The pressure on the white and black middle-class population to get out of reach by moving to the suburbs lays the foundation for a building boom there. There is even room in such a boom for a little suburban integration.

Moreover, the abandonment process clears land and relocates families "free." That will make it easier and cheaper to rebuild on a large scale. A few of the areas being abandoned are prime locations, such as Woodlawn, which lies between Lake Michigan, a huge park, the University of Chicago, and a large cemetery. And perhaps the problem of the underclass will wear itself out. In the next 15 or 20 years we may see declining birth rates among lower-class blacks as well as the violent death or incarceration of their more destructive underclass. In 20 years the inner-city may be ready for rebuilding on a scale that will dwarf the reconstruction after the Chicago fire. And there is reason to believe that such reconstructed areas could be integrated both racially and economically.

There is another side to this, however. It is a profligate society that is willing to squander the resources generations have created. In the path of advancing decay lie not only homes but job-providing factories, schools, churches, shopping centers, universities, and other institutions. The Woodlawn process may be "accomplishing" what it took a war to "accomplish" for European cities. Father Tracy O'Sullivan, pastor of a church in Woodlawn, said, "The whole Ranger thing was just one big real estate transaction." As a means of carrying out a real estate transaction, it is no more justifiable than war.

People get chewed up in the Woodlawn process. Violence is already a principal cause of death among black youths under 25. Many of the victims have been good students who had the makings of community leaders, the kind of people we were expecting to depend on in the future. And the zone of destruction is a pitiful environment for people to live in, existing day and night with the

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threat of violence and fire, surrounded by collapse. Can a society be indifferent to such a process? Can letting such a process continue any longer be considered a defensible public policy? Can the hostilities such a process breeds ever be healed?

Who are the victims?

Look closely at who the victims of the process are. There is a tendency to focus on the poor black families who are left in the neighborhood when it finally comes crashing down. Their plight is obvious and grave. But at least in material terms they have relatively little to lose. The most threatened groups are the black and white working people, who can't afford to run away when the decay process presses against their communities: the "respectable" poor, the working poor, the elderly, the abandoned, traditional poor, the residue of hard-working ethnic communities that have yet to make it into the professional and white-collar classes. It is as if the economy has played a huge trick on them. It gave them the opportunity to become modest homeowners; then it turned around and produced a process of decay that threatens to wipe out their gains, the hard-won achievements of their lives. When applied to these people, the policy of letting-what-has-been-happening-continue not only is an affront to simple justice but also produces conflict that will increase the instability of the city and harden racial prejudice. The prospect of the poor and the near-poor fighting over a diminishing supply of cheap housing in the city is not a pretty one.

But even those who can tolerate such a prospect might think twice before taking the train home, in the light of two facts: First, there is no evidence that the black lower class is getting smaller—a fact emphasized by such otherwise optimistic thinkers as Moynihan and Banfield. As Moynihan described the problem of black family instability in his celebrated "Benign Neglect" memorandum, "This problem does not get better, it gets worse." Banfield seems to think that the size of the lower class will probably increase, in absolute if not relative terms, over the next 20 years. So perhaps this problem may *not* go away. And second, Woodlawn should be a warning that it is taking less and less time for neighborhoods to collapse. The end of the historic procession of immigrant groups to the city, in conjunction with the collapse of housing economics, means that the black lower-class may wear out one neighborhood after another at a pace unimaginable a few years ago.

Even those members of the middle and upper classes, black and

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white, who can *afford* to keep running away from the problem even if it means changing cities, may not be as secure from the decay process as they think. But, more important, no family of modest means—and this applies particularly to the black middle class, struggling to establish respectable and stable communities, mostly within the limits of the central city—can feel immune from a process which could wipe out all its gains. This destroys the basic condition for a long-term resolution of America's race problem—the ability of the black middle class to develop the community, financial, commercial, and other institutions it needs so badly. Nothing is more inimical to the formation of such stable institutions than the fear that one's own neighborhood is only a few years removed from what happened in Woodlawn.

The "contribution" of government

Ironically, what happened in Woodlawn is partly traceable to public policies that were inspired by noble national ideals. For instance, the discovery in the 1930's that a third of the nation's families were ill-housed brought to light an intolerable national condition, and on the theory that every problem would yield to a program, massive public housing projects were built to replace slums. However, these provided housing for the "worthy" poor, not the families of the underclass, who were screened out or evicted. Until very recently, the majority of families in public housing were headed by working people. The areas around the public housing projects housed many of the families who had been rejected by the housing authorities as being too disorganized. An uneasy balance developed between the two groups, confined to the ghetto as they were by the wall of segregation. In the 1950's the wall cracked. Many of the working-class and middle-class black families got out —not to integrate into white neighborhoods, but to establish black middle-class communities within a still generally segregated area.

As this process accelerated, public policy focused next on the neighborhoods to which the underclass was relegated and found them unacceptably shabby. Strict code-enforcement programs were implemented to oblige the slum owner to bring apartments up to safe and sanitary standards. Housing that could not economically be brought up to such standards was to be torn down. The result, for a time, was a significant shrinkage of the housing supply. In Chicago, the pressure of code enforcement seems to have played a critical role in almost one third of all demolitions in the city between 1948 and

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1970, even during a period of extensive demolition as a result of expressway and public housing construction. Some of the code-enforcement programs were coupled with urban renewal projects, which focused primarily on middle-class areas threatened by the underclass.

In retrospect, it is apparent that public policy toward housing has been unwilling to make the class distinctions that have governed the behavior of families, white and black, who have had to live with the threat of the nearby underclass. As long as the neighborhoods to which that underclass was relegated lasted, it was possible to sustain the illusion that things were getting better. Urban renewal *did* transform Southwest Washington from slum to shiny new upper-class district. Code-enforcement programs *did* succeed in forcing many landlords to improve their buildings. Census statistics *did* reassure us that the total percentage of deteriorated or dilapidated dwelling units was geting smaller. It took Woodlawn and its counterparts in other cities to point out the catch: After 40 years of programs which ignored the existence of the underclass, the neighborhoods on which we implicitly depended to provide this class with housing were suddenly collapsing on a massive scale. Of course, we can continue to ignore or deny this issue—but it is no longer painless to do so.

The case for stabilization

We submit that the issue now is not how to end slums, but how to stabilize and upgrade them. This has a crass ring to it, to be sure. Years of rhetoric about making war on poverty have taken for granted the inspirational notion that, given the will and the proper means, everyone can be assured an "adequate income" and a "decent home." The idea of government policy consciously promoting the "stability" of slums goes against the grain; it lacks the sense of uncompromising high-mindedness which we have come to expect of national social policy. Yet it is precisely such "high-minded" and "inspirational" policies which have helped to create the plight of the Woodlawns of America. And in any event, what these devastated or about-to-be-devastated neighborhoods need isn't eradication, but rather assistance in stabilizing themselves. If the case of Woodlawn proves anything, it proves the urgency of finding ways to keep the communities that house black lower-class families from falling apart.

One reason for redesigning public policy so that its aim is to stabilize slums lies in the interests of the black underclass itself. The

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dissolution of this class, to quote Moynihan, is going to be "the work of a generation." After the early hopes of the war on poverty, the great difficulties of working with this class have become widely known. Any effort to convert the underclass into responsible, self-sufficient persons—whether through job training, school programs, welfare experiments, social work, "black capitalism," or whatnot—is undermined by their incessant drifting. Urban underclass families don't stay put very easily. This is a difficult problem even under the best of circumstances, but when neighborhoods collapse around them as Woodlawn did, they have no choice but to pack up and move on. The process of dissolving lower-class behavior depends on building up community institutions and the sense of responsibility that are the hallmark of the middle and working classes. The obvious prerequisite for this, of course, is a "community," one that lasts. Today this prerequisite does not exist.

The problem goes far beyond the suffering of people trapped in the last stages of a neighborhood like Woodlawn. The collapse of housing economics in the inner city is turning the underclass into nomads. They are the kiss of death to each successive community that they occupy. This is not to blame them in a personal sense. It is simply to state the plain fact that the economics of lower-class housing no longer work. And there is no advantage at all to the lower class in this process. If antipoverty programs must chase these people from one neighborhood to another, they will never have any meaningful impact on them.

The second part of the case for stabilizing lower-class neighborhoods stems from the interests of those who are in the path of destruction, particularly the black working and middle classes. In a city like Chicago, there are a lot of blacks who have made it. They have jobs, good incomes, homes which they own, solid neighborhoods. Some of them are becoming successful businessmen. A black investment community is beginning to form. Communities are beginning to show the capacity to respond on their own to their problems. Political power is heading their way, slowly but surely. All these gains are real, but they are recent and they are still on a very shaky basis. In general, the institutional strength of the black community is substantially less than that of otherwise comparable white communities, and this fact makes the recent gains of the blacks more vulnerable to pressure from the underclass.

The extent of this vulnerability is illustrated by the fate of a community south of Woodlawn called Chatham. Ten years ago it was the showcase of the successful Chicago black community. Today

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Chatham is becoming uneasy. Refugees from Woodlawn are beginning to filter into Chatham's big, solid apartment houses. Ten years ago a property owner in Chatham could sell "up;" today, it is said, he may have to sell "down." This short life of black middle-class communities should be of concern to everyone. The path to racial equality in this country depends on blacks achieving the same middle-class stability and power that the whites have. This takes time, concentration, and a modicum of security. The successive dissolving of communities under the pressure of the underclass deprives them of all three.

The third branch of the case for stabilizing our low-income neighborhoods is the question of justice for those of the poor, black and white, who take the brunt of the loss when a community falls apart. The rampage of the underclass has introduced a new factor into urban land economics. The prospect of a community's "going black,"and now the prospect of its subsequent mutilation, has paralyzed the market for whole sections of inner-city land. The general tendency in the American economy over the past 30 years has been for land values to rise. Even in older sections of the inner city land might easily be redeveloped (and in many places is redeveloped) for commercial or industrial or residential uses—were it not for a generalized fear of underclass "invasion." This hits blacks as well as whites, as the Chatham example shows. In practical terms, it freezes the urban land market and leaves the present group of owners holding the bag.

The present group of owners, of course, is usually the lower-middle class and working class. They own neighborhood after neighborhood of good, modest, cheap houses, decent if unglamorous apartment buildings, small business establishments, and the like. Without the threat of underclass violence it is hard to say what the market for this property would be. Possibly there would still be little demand for it; this is the belief of a number of authorities, such as George Sternlieb, who see an irreversible trend from urban to suburban living. But this view may be incorrect, and in any case has not been proved. Still, the paralysis of the market hurts whatever prospects present owners have both to maintain their communities and to recoup their investment when they want to sell.

To restore the economic viability of housing and to establish an acceptable level of personal security must be the two goals of any stabilization policy. We are not proposing that this be the full extent of national policy toward the central city. It is clear that most lower-class crime is the product of joblessness, bad education, and weak

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communal structures. But programs aimed at these fundamental problems are impossible to implement without a reasonably stable community—however shoddy from the liberal, suburban point of view—within which to develop them. Archimedes knew how to move the world, but even he acknowledged that he needed a place to stand.

The most important lesson to be drawn from the Woodlawn process, then, is that its victims are not just the inhabitants of Woodlawn. The principal victims will be the black and white working class, the lower-middle class, the traditional poor—those whom the accelerating process of decay threatens most directly. Furthermore, the Woodlawn process has become a major menace to the consolidation and growth of the black middle class itself.

The immense gains of the black population over the last 40 years —gains which the Woodlawn process menaces—were fashioned in the midst of poverty-stricken communities. The process of social advance has to start somewhere. The challenge is to assure it of a place to start. To do so will require the establishment of basic security for people and property, and more clearly focused efforts to maintain community stability in inner-city neighborhoods.

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Property taxes and populist reform

GEORGE E. PETERSON & ARTHUR P. SOLOMON

The property tax is one of the most oppressive and discriminatory of all taxes. . . . Later in the year I shall make my final recommendation for relieving the burden of property taxes.

—Richard M. Nixon, January 1972

The property tax revolt may be a major issue in the coming months. The federal government may have to step in to allow for reduction of property taxes.

—George McGovern, April 1972

It doesn't take a Gallup Poll to prove that the property tax has surfaced as a central political issue of the 1970's. The recently completed Presidential campaign established that politicians—their political antennae picking up strong signals from a rebellious electorate—have indeed gotten the message that there is widespread outrage among the citizenry at high property tax rates. As this is written, the Supreme Court has agreed to rule on whether the use of local property taxes to finance local educational expenditures violates the Equal Protection Clause of the 14th Amendment by making the quality of a child's schooling dependent upon the privately held wealth in his community. The

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outcome of that case is impossible to predict. But whatever the Court's decision, the movement toward reducing the financial reliance of public school systems on local property taxes seems too potent to reverse. In California alone, five bills have been introduced in the state legislature that would sever the connection between local property values and local school expenditures.

That relief from a burdensome tax should blossom as an issue during an election year is scarcely surprising. What is more remarkable is that the proponents of property tax relief should have succeeded so well in convincing the rest of us that property tax cuts would constitute an important tax reform of the "new populist" variety. For in current discussions the property tax has gained the reputation of a tax whose burden falls most heavily on the little man. In the Wisconsin primary, for example, Hubert Humphrey, borrowing language from William Jennings Bryan, went so far as to describe the fight for property tax relief as part of an ongoing battle between "the people of Wisconsin" on one side and "the international financiers, the credit companies, the giant banks, the bastions of the wealthy, and the giant corporations" on the other side.

Yet beneath this populist facade, the fact is that in state after state the principal leaders of the property tax "revolt" prove to be substantial homeowners and realtors rather than the proverbial "little men." We believe that it is these relatively well-to-do people who would be the true beneficiaries of currently popular proposals for effecting massive property tax reductions. This is not to say that the grievances lodged against the property tax are fictitious. On the contrary, the present variation in property tax rates as between poor and wealthy communities undoubtedly works to the detriment of the poor, and for some families, like elderly homeowners on fixed incomes, the property tax represents a true hardship. However, if the intent were to frame a fair tax system, these inequities could easily be remedied within the property tax structure. A shift to statewide property taxation, for example, would remove the interjurisdictional inequities, and tax exemptions could be used to ease the burden on elderly homeowners at a relatively small cost to the state or federal government.[1]

Instead, we seem to be drifting into a situation where *replace-*

[1] The Advisory Commission on Intergovernmental Relations has placed the cost to the federal government of such provisions at about $1 billion. This compares with the $15 billion of property tax relief that was discussed during the election campaign.

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ment of the property tax by some other means of collecting revenue, either at the state or federal level, will be offered as a sop to those who desire genuine tax reform. In fact, the property tax has become such a symbol of taxpayer antagonism that its replacement may go some way toward satisfying those whose real complaint is that there is "too much" government and "too much" taxation—even if the substitution of some other revenue source for the property tax were to leave total tax collections unchanged.

In this essay we want to take one step toward clarifying the debate over property tax reductions, by assessing the reform credentials that property tax relief legitimately can lay claim to. In the first section of the essay we show that currently popular proposals for having the state or federal government take over the *educational* portion of local property taxes could easily *increase* rather than reduce the regressivity of the tax system. If property tax relief could be extended to cover *all* local services, the outcome would be more equitable. Unfortunately, however, the lower courts have held that the constitutional arguments for severing the link between local property wealth and local public service quality apply uniquely to schooling.

In the end, the contention that property tax relief by itself would constitute an important tax reform rests upon the inequities that allegedly are associated with the choice of property, especially residential property, as the object of taxation. The residential property tax commonly is thought to impose unfair tax burdens on both single family homeowners (who pay about 50 per cent of all property tax revenues) and poor people (who by most measures see the highest proportion of their incomes disappear in property tax payments). The latter sections of our essay seek to assess this commonly held opinion as to the effects of property taxation. We show that once the entire tax system is taken into account, single family homes are not "overtaxed," and that, if fairly administered, the property tax would not be regressive. If this is conceded, then the demand for reductions in the property tax amounts to little more than a demand for middle-class *tax relief,* pure and simple, which could equally well be accomplished by reductions in any other tax instrument.

Who pays how much for what?

By now it seems clear that if we are to have large-scale property tax relief, it will take the form of state or federal financing of a por-

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tion of local *educational* expenditures. In principle, this approach seems to command bipartisan support. In his Presidential campaign, Senator McGovern called for the federal government to assume one third of the total costs of local public education so that local jurisdictions could cut their property tax rates. Both the President's Commission on School Finance and the Advisory Commission on Intergovernmental Relations have urged President Nixon to encourage full *state* funding of education, with one proposal being that all local taxes which currently are used to raise educational revenue (principally property taxes) be shifted to the state level. Recent court decisions hold out the possibility that some such shift in educational financing will be made mandatory.

In Table 1 we have illustrated, using the Boston metropolitan area as our example, the relationship between total property tax rates and property tax rates for educational purposes which typically holds for the country at large. As can be seen from the third column, Boston's low-income communities pay *total* property tax rates that are more than one-third greater than the tax rates paid by high-income suburban communities in the area. In this respect, the Boston area is representative of the country as a whole. Nationwide, the median property tax rate in the central cities exceeds that

TABLE 1. *Effective Property Tax Rates—Boston Standard Metropolitan Area, 1970-1971*

	Median Income For Families[1]	Educational Expenditure Per Pupil	Total Effective Property Tax Rate[2]	Effective Tax Rate For Educational Purposes[2]
Low Median Income				
Boston	$ 9,133	$ 832	4.86	.97
Somerville	9,594	688	4.98	1.39
Malden	10,204	643	4.06	1.06
Revere	10,718	800	4.07	1.38
Quincy	11,091	845	3.72	1.53
Waltham	11,842	841	2.78	1.31
Simple Unweighted Mean			**4.08**	**1.27**
High Median Income				
Belmont	$13,559	$ 840	2.92	1.34
Milton	14,728	942	2.53	1.09
Newton	15,381	1,116	2.70	1.30
Lexington	17,558	967	3.61	2.24
Wellesley	19,401	1,109	3.14	1.70
Weston	23,530	1,190	2.94	1.82
Simple Unweighted Mean			**2.97**	**1.58**

1 Source: 1970 U. S. Census of Housing.
2 Derived from M.I.T. Urban Economics Project. These are tax rates based on true market value.

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in the surrounding suburbs by about seven per cent. While this difference may seem slight, once low-income suburbs are grouped with the central city, as in Table 1, the difference in tax rates between these jurisdictions and the wealthy suburbs becomes much more pronounced. This simply reflects the fact that in their fiscal circumstances the lower-income communities that lie in the inner ring around major cities resemble the central city far more than they do the affluent suburbs, so that treating "suburb" as a single category is bound to obscure tax rate (as well as other) differentials.

Now in all jurisdictions, rich and poor, education represents much the largest single expenditure item financed from local property tax revenue. It might seem, then, that relieving local districts of a substantial share of school costs would have to reduce property taxes most in the big cities where these rates are highest. Certainly, this is the line of reasoning that has been adopted by the courts. In *Serrano v. Priest,* for instance, the California Supreme Court held that "the California public school financing system, with its substantial dependence on local property taxes . . . *invidiously discriminates against the poor* . . ." (emphasis added). The court went on to argue that it was necessary to break the nexus between local wealth and local school expenditures, not as an abstract exercise in equitable classification, but in order to do justice to *poor* families, such as the plaintiffs in the *Serrano* case, who were obliged to pay higher tax rates for schooling because the homes in their neighborhood were modest ones. The court repeatedly contrasted the $5.48 per $100 of assessed valuation which the generally low-income families of Baldwin Park had to pay in school taxes with the tax of $2.38 per $100 paid by the residents of affluent Beverly Hills.

For the nation as a whole, however, as well as for the Boston area communities illustrated in Table 1, the court's contrast between rich Beverly Hills and modest Baldwin Park proves to be misleading. Property tax rates for the support of schools actually tend to be lower in the central cities than elsewhere. Effective property tax rates for educational purposes in the various Boston area communities are shown in the fourth column of Table 1. Note that Boston's school tax rate is barely half the rate levied in the area's richest suburbs.

There is an ambiguity in the term "poor" that seems to have proved troublesome to the courts. If "poor" communities are those with relatively little taxable property per capita, then of course the "poor" must tax themselves at a higher rate than the "wealthy" in

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order to provide any given level of expenditure.[2] But if we use "poor" in its ordinary acceptation to refer to households that have low incomes, the conclusion that poor communities must pay higher property tax rates than wealthy communities in order to finance equal expenditures no longer follows. The amount of taxable property that a community possesses depends as much on the concentration of industrial and commercial property within its boundaries as on the opulence of its homes. While the much-touted exodus of industry to the suburbs has reduced the tax base advantage of the central cities, the cities still tend to have more wealth per capita to tax than do other jurisdictions. This means that the major cities often are able to finance a given level of educational expenditure at a lower tax rate than the suburbs, despite the fact that the households living in the cities are less affluent. If cities and suburbs were called on to furnish their citizens with the same mix of public services, the greater tax base of the cities would show up in lower *total* tax rates as well. This result is not observed only because the cities are obliged to provide levels of police and fire protection and of health and hospital services which far exceed those that have to be financed by most suburbs.

A regressive step

From the point of view of progressive tax reform, education is the worst of all possible services to bring under a regime of full state or federal funding. Interpreted literally, Senator McGovern's campaign proposal for federal funding of one third of the total costs of education in each community would involve a major redistribution of public funds in favor of wealthy jurisdictions, where present spending on schools is highest. Any program to trim local school taxes by a uniform percentage, which President Nixon at times seemed to advocate, would have the same regressive effect, since the highest school tax rates also are paid by affluent suburbanites. Local property tax rates for welfare, police and fire protection, or health and hospitals are inversely related to family income (so that cutting present property tax rates by some fixed fraction would benefit most the poor taxpayer); but local tax rates for schooling are

[2] Even this may not be true, if communities with lesser per capita wealth also have fewer people who require the public service. For example, the number of children enrolled in public schools per thousand total population is much lower in the central cities than in the suburbs. This would make it possible for the cities to finance equal per pupil expenditures at the same tax rate, even with a lower tax base.

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positively related to family income (so that cutting local taxes in this way benefits most the affluent taxpayer). In effect, present proposals for granting property tax relief by shifting the cost of financing schools to a higher level of government single out for favorable treatment the one part of the public sector—education—where the well-to-do suburbs typically concentrate their revenue efforts.

Table 2 summarizes the present relation between school expenditures, per capita income, and property tax rates for a sample of 649 school districts, representing 19 per cent of the nation's population. The school districts were chosen from seven states (Delaware, North Carolina, Washington, Michigan, California, New York, and New Hampshire) which represent virtually the entire range of degrees of state participation in school financing.

TABLE 2. *Educational Property Tax Rates and Per Capita Income for Cities, Suburbs, and Rural Areas, 1971*[1]

	CENTRAL CITIES	SUBURBS	SMALL CITIES	RURAL AREAS
1. Per capita income	$2,682	$3,082	$2,431	$1,974
2. Education expenditures (dollars per pupil average daily attendance—ADA—unweighted average)				
Total	$1,054	$ 896	$ 773	$ 750
Federal	67	26	51	57
State	383	372	334	401
Local	604	498	388	293
3. Local property tax rate for education (unweighted average)	1.01%	1.46%	1.12%	1.03%
4. Local property tax rate presently needed to raise each $100 of local revenue per pupil ADA	.17	.29	.30	.35

[1]Source: Betsy Levin, Thomas Muller, William Scanlon, Michael Cohen, *Public School Finance: Present Disparities and Alternatives*, Vol. 1 (Urban Institute: 1972).

The differences in educational expenditures shown in line 2 of the table reflect salary differentials more than anything else, and are not to be interpreted as demonstrating that the quality of schooling in central cities is superior to that offered elsewhere. Nevertheless, the last two lines of Table 2 indicate that in purely financial terms low-income jurisdictions enjoy a considerable tax advantage. At present, local property tax rates for educational purposes (line 3) bear a discernible direct relation to community income. They are lowest in cities and rural areas and highest in the suburbs.

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Line 4 of the table indicates the local property tax rates which presently are needed to raise $100 of state and local per pupil revenue in the different types of jurisdictions. We may assume that since much of federal funding goes to finance compensatory education for minorities or to provide remedial education for children with learning handicaps, this aid will continue to be distributed on the present terms whatever financing arrangements are adopted by the states. The different tax rates in force at the present time may then be contrasted with the single tax rate, somewhere in the middle of the range, that would be required to raise $100 per pupil revenue if the link between local wealth and local school spending were broken.

Proposals to make every community in a state pay the same tax rate in order to raise $100 of school funds have been dubbed "power equalization" plans. The plaintiffs in the *Serrano* case seemed to be arguing in favor of such a power equalization formula on the grounds that it would vastly improve the relative fiscal position of poor jurisdictions. However, the redistributive impact of power equalization is by no means clear. Table 2 shows that on average taxpayers in rural areas would experience the greatest improvement in their school purchasing power. In contrast, the amount that central city taxpayers have to pay per $100 of school revenue on average *would increase far above present levels.* It is not possible to say whether the shift to power equalization on balance would be "pro-poor" or "pro-wealthy," but there can be no doubt that it would work strongly against the interests of the central cities, which usually are thought to suffer most from the present property tax system.

It is possible, of course, to devise a form of state or federal takeover of responsibilities for school financing that *would* be progressive. Any formula which called for providing significantly more education money per pupil to low-income districts than to high-income districts would be likely to have net progressive effects, since the progressive distribution of school aid would outweigh the regressive redistribution of tax burdens. But tax "relief" of the kind that involves across-the-board cuts in school tax rates runs the grave risk of severely increasing the regressivity of the tax system.

Public services and the Fourteenth Amendment

From the point of view of fiscal equity, the moral of the preceding section is that property tax relief could be distributed more progressively if some fraction of all local expenditures were shifted to a

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higher level of government, instead of a fraction of educational expenditures only. The courts, however, have taken the position that the constitutional argument for severing the link between local wealth and the quality of local services applies uniquely to schooling. In each of the school finance cases resolved to date, the lower courts have been careful to distinguish education, as a "fundamental interest," from other local public services, which, however important, are not deemed to be "fundamental" in the constitutional sense. In *Van Dusartz v. Hatfield,* the Federal Third District Court of Minnesota argued that classifying citizens according to wealth, as occurs when citizens are grouped together in rich towns and poor towns, is inappropriate for purposes of determining educational expenditures. But the court stated explicitly that this finding was not meant to serve as an "opening wedge for eventual fiscal neutrality in all governmental services through the medium of the 14th Amendment . . ." The court was willing to concede that welfare payments, for instance, hold immense importance for the recipient, but nonetheless asserted that this interest is not fundamental in the constitutional sense of being "basic to the functioning of a free society" in the same way education, voting rights, or criminal rights are.

To a layman, the court's reasoning on this score must remain something of a mystery. In *Van Dusartz,* the Minnesota court, citing the *Serrano* case, emphasized that it is not the " 'importance' of [the] asserted interest" alone which renders education a fundamental interest. Yet in *Serrano* itself, the court seemed to assert just the opposite, maintaining that education merited special legal protection because "it is perhaps the most important function of state and local governments." Legal reasoning aside, however, it is clear that the courts for one reason or another are not ready to apply standards of fiscal neutrality to local services other than education. Of course, nothing in the school finance decisions to date would prevent the states or the federal government from taking over the responsibility for financing additional local services if they thought this desirable. But the decision to intervene in financing services other than education apparently will have to be a political one, made without prodding from the courts.

Are property taxes too high?

To this point we have been occupied with the question of differences in property tax rates among communities. We have suggested that adoption of a uniform statewide property tax financing

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a wide spectrum of public services would represent a significantly more progressive system of taxation than the present one. There remains, though, the question of whether a statewide property tax would be an appropriate instrument for raising revenue even if this could be done, or whether true tax reform requires scrapping the property tax altogether. The general view seems to be that no matter how fair average community tax rates may be, the property tax always will lay an unfairly heavy burden on certain types of families. These victims of overtaxation usually are identified as single family homeowners, on the one hand, and poor people, on the other.

There is a fundamental principle of taxation which holds that, because taxing a commodity will discourage its consumption, taxes insofar as possible ought to be imposed on "bads" (that is to say, commodities which the community regards as undesirable) and spared on "goods" (commodities which the community regards as especially valuable). John Stuart Mill made one of the first practical applications of this principle when he argued that the English threepence tax on window glass was in truth a tax on sunlight, the consumption of which the government ought to be encouraging instead of making more costly. To many modern writers, the single family home seems to occupy the same position as an undoubted "good" that Mill assigned to sunlight. Testifying before the Joint Economic Committee of the Congress, former Assistant Treasury Secretary Murray Weidenbaum took this point of view:

> One of the ways of halting the growing blight of our cities, I believe, is to foster homeownership, to encourage the individual family to have a stake in its housing. Hence, I would be very reluctant to support, at this time, reducing, much less eliminating, the present [favorable federal income] tax treatment of homeownership.

Does the property tax truly "overtax" the single family home? The essential facts regarding the homeowner's property tax burden are these: In 1969 some $30.7 billion was raised from property taxation, almost half of which came from single family homeowners. Although homeowners pay less than half the total property tax bill, they are in many ways the most conspicuous victims of rising property tax rates because, unlike business enterprises (which pay approximately 40 per cent of the property tax) or owners of rental property (who pay less than 10 per cent), the homeowner cannot escape any part of the tax burden by passing it on to third parties.

For the nation as a whole, updated census data reveal that in 1969 single family homes were taxed at an average effective rate

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of 1.8 per cent of true market value.[3] Those who charge that single family dwellings are overtaxed almost always compare this average residential property tax rate with the sales tax rates which are levied on other commodities. In this comparison, the right to occupy a home is treated as a "good," like any other good, which the consumer pays for with his monthly check to the landlord. If the consumer owns his own home, he is thought of as charging himself an implicit rent equal to the sum which he could earn by renting his property to someone else. The total amount which a consumer pays in property taxes then can be analyzed as if the same sum had been collected in the form of a sales tax imposed on rental transactions. For instance, if a home rents for $200 a month and has a total market value of $20,000, the average property tax rate of 1.8 per cent is exactly equivalent to a sales tax of 15 per cent imposed on rents: The property tax raises $360 a year or $30 a month, just as would a 15 per cent sales tax on rents. Of course, for single family homeowners the sales tax equivalent of the property tax is a rather fictitious notion, since no "sale" takes place between the owner as landlord and the owner as tenant. It is always possible, however, to calculate the market rent for comparable housing and to estimate the sales tax equivalent of the homeowners' property tax rate based on rents for comparable housing.

When Dick Netzer carried out computations of this type on 1960 census data, he found that the average residential property tax was equivalent to a sales tax on rents of 17 per cent.[4] As Netzer pointed out, this is higher than the average sales tax on virtually all other commodities, except cigarettes, alcoholic beverages, and gasoline. Insofar as the claim of "overtaxation" of single family housing reflects anything more than homeowners' irritation at rising tax burdens, it seems to be based on this kind of comparison, which shows that the property tax—viewed as a sales tax—weighs more heavily on housing than comparable sales taxes do on other commodities.

If housing expenditures are treated harshly by the property tax,

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[3] This is calculated on the basis of an estimated $11,962 million paid in property taxes on single family homes and an estimated total value of single family residential structures of $674 billion. The authors are indebted for this estimate to Helen F. Ladd, "An Analysis of the Property Tax in the Context of the American Fiscal System," Harvard University, mimeo, March 1972.

[4] Dick Netzer, *Economics of the Property Tax* (Brookings Institution, 1966). This is now considered the standard work on the property tax. Professor Netzer has pointed out in private correspondence that more recent estimates of the sales tax equivalent of the property tax place it at 20.9 per cent for 1970. Insertion of this figure in Table 3 would provide stronger evidence for the contention that housing is "overtaxed."

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however, they enjoy exceptionally favorable treatment at the hands of the federal income tax. Both the interest component of mortgage payments and the property tax payment itself are deductible from the federal income tax base. Once these deductions are considered, the rate of taxation on single family homes is substantially diminished. Table 3 shows the details of a comparison of sales tax rates for housing and all other goods. The point of the comparison

TABLE 3. *Net Tax Rates on Housing vs. Other Commodities*

	HOUSING	OTHER COMMODITIES
1. Average sales tax or equivalent	17%	6.4%[1]
2. Tax that is deductible from federal income tax base	17%	4.2%[2]
3. Average marginal rate of federal income tax	22%[3]	22%[3]
4. Reduction of net sales tax rate due to income tax deductibility (2) x (3) of sales tax	3.7%	.9%
5. Average interest payments, as per cent of rental value or value of commodity purchases	28%[4]	1.7%[5]
6. Reduction in net sales tax rate due to income tax deductibility of interest payments (5) x (3)	6.2%	.4%
7. Net Tax Rate (1)-(4-)-(6)	**7.1%**	**5.1%**

1 Calculated as total 1969 excise tax receipts (Federal Government, $15.2 billion; State Government, $24.1 billion; Local Government, $6.6 billion: Total $45.9 billion) as a percentage of personal consumption expenditures ($577.5 billion) plus gross private domestic investment ($139.8 billion).

2 State and local sales taxes only.

3 From Henry Aaron, "Income Taxes and Housing," *The American Economic Review* (December 1970). The mean marginal rate of all taxpayers in 1966 was 19 per cent, and homeowners as a class have above-average incomes.

4 Calculated as percentage of total rent going for interest payments among owners of rental property. From U.S. Department of Housing and Urban Development, *Study on Tax Considerations in Multi-Family Housing Investments* (1971).

5 Assumes that interest at eight per cent was paid on the $122.5 billion of consumer credit outstanding in 1969. The $9.8 billion of interest charges represents 1.7 per cent of total personal consumption expenditures ($577.5 billion).

is simple. Once we take into account federal income tax provisions, the sales tax equivalent of the average property tax rate becomes quite comparable to the average sales tax rate that is imposed on other commodities. The tax rate on housing may be slightly higher, as indicated by the last line of Table 3, or it may be slightly lower, as could be obtained by varying slightly some of the assumptions expressed in the footnote. But all in all, housing seems to be taxed at about the same rate as other commodities. It is true that, when viewed as a sales tax equivalent, property tax rates in some states —like Massachusetts and New Jersey—seem very high. But the

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alleged overtaxation of dwelling units is a frail reed upon which to base a nationwide campaign for property tax reduction.[5]

The regressivity of property taxation

We have saved the most substantial indictment of the property tax for last. This is the contention that the property tax is a regressive tax whose burden falls most heavily on poor people. We must be careful right here to distinguish the regressivity, if any, that is inherent in the choice of residential property as a tax base from the regressivity that attends the current administration of local property taxes. As we have seen, the present differences in average tax rates among communities work on balance to make poor families pay higher property tax rates than wealthy families. This source of regressivity, of course, would be eliminated with conversion to a statewide property tax. Similarly, there is mounting evidence that local assessments are carried out in such a way that low-income properties are assessed at a substantially greater proportion of true market value than high-income properties. This means that within the same city poor homeowners often are saddled with higher effective property tax rates than homeowners who are better off. Presumably, this source of regressivity, too, could be eliminated with the adoption of uniform statewide assessment practices. (Another source of regressivity—probably the most important—is the deductibility of property tax payments from the federal income tax base. However, any state or local tax that replaced the property tax would likewise be deductible from the income tax base.)

Whatever regressivity is *inherent* in residential property taxation must be traceable to the fact that housing is a "necessity" which appears to account for a decreasing share of family budgets as income levels rise. Repeated studies have shown that families in low-income brackets spend a much higher proportion of their income on housing than do families with higher annual earnings. According to the President's Committee on Urban Housing, households that earned less than $2,000 in 1960 paid well over a third of their income for rent, while the majority of households that earned

[5] Another standard for comparison in deciding whether the property tax rate on single family homes is "too high" or "too low" is to consider the property tax as a substitute for an income tax on the "implicit income" that is earned by investing one's wealth in a single family home. Helen F. Ladd, *op. cit.*, has shown that viewed in this light too, the average property tax rate is roughly comparable to the average income-tax equivalent which would be imposed if the implicit earnings from owning a single family home were subject to the federal income tax.

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over $10,000 paid less than 10 per cent of their income for rent. If we assume that the sales tax equivalent of the property tax is the same at all rent levels, then as long as rents decline as a proportion of income as families get richer, property tax payments also must account for a decreasing share of family income as earnings grow.[6]

This conventional explanation of the property tax's regressivity rests on some questionable assumptions that bear examination. First, it is assumed that landlords can shift the entire burden of the property tax to the tenant in the form of higher rents. This is not the place to rehearse arguments concerning the incidence of the property tax. Suffice it to say that recent studies, both theoretical and empirical, have shown that the owners of property must absorb a sizable portion of property tax payments. A careful study conducted in San Francisco demonstrated that when the Petrix-Knob bill altered assessments on properties in the San Francisco area, well over half the increase in effective property tax rates that resulted was absorbed by the owners of these properties, rather than passed on to tenants. In general terms, *the greater the uniformity of property tax rates among jurisdictions in a metropolitan area, the greater the proportion of the property tax burden that must be borne by landowners.* Over the long run, the mechanism that permits landlords to pass on property tax payments to tenants is the threat that, unless higher rental rates are forthcoming, the owners will construct apartment buildings in other jurisdictions where the tax burden is lighter. Meanwhile, they will allow dwelling units in the high-tax town to run down without undertaking improvements that would increase the value of their property and so their tax bill. However, if property tax rates are uniform everywhere and no low-tax havens exist, the real estate investor cannot shift his funds in this way, and must absorb the property tax out of profits. Since landlords, on the average, earn much higher incomes than their tenants, any absorption of the real estate tax by landlords would make the property tax system more progressive than is implied by conventional discussions, where it is assumed that the whole tax burden is borne by the tenants.

Note that *one important consequence of the imposition of a uniform statewide property tax would be to increase the proportion of the tax which is paid by the owners of capital.* As argued above, that portion of the property tax rate which is common to all property

[6] Actually, the ratio of housing-value/annual-rent can be shown to increase with rent level. This means that the property tax is equivalent to a sales tax with a progressive rate structure, which taxes low-income housing at one rate and luxury housing at another, stiffer rate. This adds to the progressivity of property taxation.

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holdings cannot be shifted forward to tenants. By eliminating present differences in municipal tax rates, statewide property taxation would increase the common floor of property tax rates and, over the long run, would increase the share of the property tax burden that is borne by landlords. Substituting a statewide property tax for local property taxes, then, should have two strongly progressive effects: 1) It should lower the effective tax rate in low-income communities and raise the rate in high-income communities, and 2) it should increase the proportion of the property tax that has to be paid by the owners of real estate, who usually are richer than their tenants.

Even if the burden of the property tax could be shown to fall on the occupants of housing, rather than the owners, it would by no means be clear that the overall effect of the tax is regressive. The standard argument which purports to establish property tax regressivity relies on calculations of rental expenditures as a per cent of current incomes showing that housing expenditures do not increase very much with family income. Most housing analysts agree, however, that families make decisions regarding the purchase of expensive consumer durables, such as homes, on the basis of their expectations of normal or long-term income rather than on their current earnings. A young married college graduate with a small family and an annual salary of $15,000 may purchase a $40,000 home if he expects his future earnings to grow, whereas a middle-aged household with the same earnings but no prospect of further income growth may live in a home of much less value. In any given year, some of the households which show very low current incomes are likely to be temporarily out of work, or to be headed by small businessmen suffering a bad year. These families will have adjusted their housing consumption to their long-run income expectations. The result is that for these families current rents represent a misleadingly high proportion of current incomes, since their incomes are abnormally depressed in the current year. Several studies have shown that once a "normal" or long-run income concept is used, housing expenditures more or less keep pace with family income. Over the long run, then, even the portion of the property tax that is shifted forward to the tenant would have almost the same effect as a constant-rate tax levied on income.

Tax reform or tax relief?

In any campaign for tax reform, one of the most difficult tasks is to distinguish the self-interested desire to secure tax relief from prin-

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cipled objections to the tax structure. None but the most politically naive would put forward an argument couched in terms of naked self-interest if more respectable arguments about tax justice would lead to the same conclusion. In this essay we have attempted to evaluate the charges that the property tax overburdens central cities, single family homes, and poor people. Our conclusion is that the selection of residential property as the tax base, in combination with the federal income tax, does not overburden housing as a class relative to other goods, nor need it bear hardest on poor people, especially when there is a uniform statewide property tax. Moreover, we have discussed numerous ways in which property tax relief could make our system of taxation *more* regressive. If federal assistance were used simply to absorb some fraction of present levels of local school spending, or all property tax rates for education were trimmed by a uniform percentage, the benefits from tax relief would be highly concentrated on upper-income families in the suburbs. From their vigorous lobbying for property tax cuts, it would seem that substantial numbers of homeowners already have reached this conclusion. Those who truly desire to make our property tax system more equitable should resist these and other illusory "reforms," such as replacement of property taxes by a national value-added tax or increased state sales taxes, and should encourage instead enactment of a system of uniform statewide property taxation that extends in some measure to *all* basic public services.

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Age, sex, marriage, and jobs

CAROLYN SHAW BELL

OVER the past three years, while the unemployment rate has soared from 3.5 to six per cent and stubbornly refuses to decline significantly, the role of women in the economy has received special attention. The spectacular rise in the number of women who have joined the labor force has been singled out as an important structural change in the supply of labor—a change which explains, or helps to explain away, the existence of large numbers of people without jobs.

It is all very reminiscent of the late 1950's, when the theory of "structural" unemployment first made its appearance. At that time, persistently high rates of unemployment were attributed by some to shifts in the economy which changed the structure of demand. These observers claimed that swift changes in technology had outmoded the skills of many workers and that the decline in agricultural employment and the burgeoning of the service and government sectors had reduced the demand for people with minimal levels of education and training. But there was also an opposing interpretation, which held that the deficiencies of demand were not structural but aggregate, and that the goal of policy should be to stimulate the entire economy rather than to provide training programs for the disadvantaged. In terms of pure economic analysis, the question as to which explanation best fitted the situation of the 1950's was never

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answered, but the change of administration in 1960 settled the policy problem. The choice was for increasing aggregate demand—and the policy worked.

The situation today, with unemployment now attributed to changes in the structure of supply, or to changes in the composition of the labor force, deserves critical attention not so much because of the immediate political issues involved as because of what it implies about our abilities to look at the real world. The Nixon Administration has attempted to minimize the significance of persistently high unemployment by presenting a picture of working women that can be described, at best, as distorted and, at worst, as lacking any theoretical foundation whatsoever. For unlike the analysis of 15 years ago, the current argument does not claim that it is a lack of experience, of marketable skills, or of potential productivity that plagues today's unemployed workers. Instead, these are identified simply as women and young people—as if such categories as sex and age indicated direct causation rather than mere classification.

This argument, which has also found widespread acceptance among economists in academic and professional circles outside government, runs as follows: Women and teenagers account for a remarkably larger proportion of the total labor force now than in the past, and these groups are "traditionally" subject to higher rates of unemployment than are adult males (especially white adult males). It follows that the overall rate of unemployment rises in response to the increasing numbers of women and teenagers entering the job market. Fortunately, the economic role of these people differs from that of adult men (who form the largest part of the labor force), so that their higher rate of joblessness does not imply serious loss either of social welfare or of economic output. The rationale has nowhere been spelled out to the degree of precision these sentences suggest, but it has been hinted at with more or less subtlety by various spokesmen and analysts.

The most blatant error in this line of reasoning is that the composition of the labor force has not, in fact, changed very much during the recent period in which high unemployment rates have become a matter of concern. The significant alteration in patterns of employment has occurred over two decades, not over three years. True, in 1950 the number of women working or looking for jobs amounted to 34 per cent of all those over 16, whereas by late 1972 this figure had grown to 44 per cent. However, looking at the civilian labor force from 1969 to the present—the period when unemploy-

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ment rose so sharply—we find that men accounted for 62.0 per cent of the total in 1969, 61.8 per cent in 1971, and 61.7 per cent in 1972.

A more significant lapse in the reasoning is the assumption that "historically" or "traditionally" high rates of female unemployment can naturally accompany a sharp deviation from the "historical" or "traditional" pattern of female employment and labor force participation. In fact, the discrepancy between unemployment rates for men and those for women that developed after 1962 does not resemble the earlier patterns. From 1947 to about 1962, unemployment among both sexes rose during business slumps at about the same rate. During periods of recovery, employment gains differed between the sexes. The unemployment rate for men typically dropped more sharply and to a slightly lower level than that for women. But the differences were minimal, compared to the divergence that has persisted during both the expansion of the 1960's and the recession which began in 1969. To speak, as some do, of the "historically higher unemployment among women than men" poses a problem in defining history.

The "carrot" or the "stick"

Finally, the argument about the significance of women's unemployment which follows from the notion that the economic role of working women differs essentially from that of men contains a basic fallacy. This is exemplified in the point of view of former Secretary of the Treasury Connally. Testifying before the Joint Economic Committee in February, he pointed out that a six per cent unemployment rate is not as critical today as in earlier decades, because many of the jobless then were homeowners, while today most of the people without jobs are women. This particular juxtaposition of homeowners versus women defies economic analysis. In an article in *Life* on June 30, 1972, Mr. Connally defended present economic policies and promised lower unemployment by the end of the year for all, "including liberated women seeking their first job and many others who would not be in the labor market if there were not a flourishing economy."

These quotations express what has been called the "reserve army" explanation of women in the labor force but might also be termed the "carrot" theory. As the economy expands and jobs become plentiful, it is thought, more women enter the labor market. Then, although total employment is rising, unemployment as a percentage of the labor force may also rise because of the greater num-

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bers flocking in to look for jobs. Because this unemployment is "caused" by prosperity, it does not pose a severe economic problem.

It is equally possible, of course, to construct from the same data an alternative explanation: that women work under the "stick" of economic necessity and that their contributions to family income may enable the homeowners to meet their mortgage payments. In August 1972 almost five million people were unemployed, and of these only 729,000 had never worked before. These "new entrants to the labor force" included 46,000 adult men and 87,000 women, a ratio that has not changed very much over time. The vast majority of the new entrants, of course, were teenagers leaving school, and among that group males substantially outnumber females. In short, among the 1.7 million unemployed women over 20 years old, only five per cent are job hunting for the first time. They share this so-called "liberation" with about three per cent of the unemployed men in the same age group, who are also seeking their first jobs. The largest group of unemployed people have lost their previous jobs. Of the adult women without work, 38 per cent had been laid off; of the adult men, 61 per cent. The percentage of unemployed women who have *left* their last position exceeds that for men because most married women adjust their work to their husbands'. If the men move, their wives give up their jobs and start looking again.

Along with the notion of women as a "reserve army" of manpower goes the conviction that most women look for part-time jobs, which are somehow peripheral both to the economy and to the family. This idea provides a rationale for the conviction that the economic role of working women differs significantly from that of men. But again the facts contradict the myth. While more women than men work only part of the year or less than 40 hours a week, only one out of five does so by choice. Of the 33 million women in the labor force, only six million are on a *voluntary* part-time basis, and the proportion seeking full-time work has not varied much over the past 15 years. *Involuntary* part-time work, including short workweeks, layoffs for a day or two, and the like, greatly exceeds 20 per cent; like recent rates for unemployment, these figures run considerably higher among women workers than men.

The "carrot" interpretation attributes women's work to their desire for "pin money," or for "the little extras," or even "for a change," "to get out of the house," and other not very impressive motives. This line of thinking leads to a serious neglect of two developments resulting from the substantial increase in the number of employed women over the past two decades.

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The first area of neglect is the contribution of women to national production and output. Since 1950 over 14 million women have been added to the total labor force, obviously representing a sizable labor input. This takes on added importance in view of the declining labor force participation rates among men. Although 87 per cent of the male population over 16 years of age was in the labor force in 1950, by early 1972 the figure had dropped to only 79 per cent. Over a 20-year period the total working population grew from 64 million to 87 million, but 14 million of the new workers were women, compared to only nine million additional male workers. During the same period, real national output has more than doubled, something which could never have occurred if women had *not* entered the labor force in rising numbers. Despite the productivity increases which took place, an economy dependent solely on the male labor force would have experienced a measurably smaller expansion than the growth of national product which actually ensued. None of these data lend much credence to the "reserve army" explanation of women at work.

Women's earnings and family income

The other significant oversight consists of ignoring the impact of women's earnings on family income. Here again there have been more or less subtle attempts to minimize the sums involved or their critical importance to particular families. In his *Life* article, after his one-sentence dismissal of female unemployment, Mr. Connally turned to what he clearly believes to be the dominant contributor to economic welfare, asking: "Meanwhile, what's happening to the working man and his family?" The implication is clear: The working *man* has a family to support; his employment does not stem from attempts to be liberated or other suspect lures. Nor is this kind of interpretation confined to members of the Administration; the equation of "married men" with "breadwinners" is almost as widespread among economists as the equation of "woman" with "housewife," "wife and mother," or (whether or not she fulfills this potential) "prospective wife and mother." This persistence of the simplistic notion that women's earnings are merely supplementary does great harm not only to attempts at understanding the present unemployment situation, but also to the design of economic policy in many spheres.

It is true that married men account for 47 per cent of the total labor force; they number 39 million. But aside from the 18 million married women in the labor force, there are another 13 million women who maintain households of their own, and who lack husbands

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to provide any income. In these cases, the woman is head of the household. In March 1972 such families included 10 million children under 18, and reported a median income of $4,456, compared to a median income of $11,810 for families with two parents as well as children. In the face of such data, it is difficult to countenance the repeated assertions that married men constitute some sort of "primary economic group," at least as far as human welfare is concerned.

As for the financial responsibilities of married men, in 1970 over half of those in the labor force had wives who contributed earnings from employment. Married working women provide over 25 per cent of the total income in this country, but to most of their families, of course, they bring much more. For example, in 1970 over five million wives earned between $3,000 and $5,000; more than half of them were married to men whose own employment brought in less than $8,000. One out of four working wives provides over 40 per cent of her family's total income; two and one-half million husband-wife families rely on the woman's earnings for over half the total income. Given these figures, what sense does it make to refer to married men as the "breadwinners"?

Moreover, aggregate figures necessarily blur details which can be crucial to individual families. A wife's earnings, even though they amount to less than half the total family income, can forestall debt and bankruptcy in the case of emergency. Depending on the size and composition of her family and on the other income available, a wife's wages can lift a family out of poverty. Since 1959 the number of poor families headed by a man has decreased by over half, while the number of poor families dependent on a woman has increased. The reason becomes clear from a simple comparison, over this time period, of families at various levels of income. In each of the census years 1950, 1960, and 1970, about 12 per cent of the families in the lowest fifth of the income distribution consisted of married couples with a wife earning some income. But among families higher up on the economic scale, this percentage changed drastically over these 20 years. Among those in the *top* three fifths of the income distribution, the number of families with working wives grew by over 75 per cent. Women who work have moved their families out of the "middle income" classes; in the top five per cent of the income distribution, women earners now contribute to 40 per cent of the households as compared to about 15 per cent 20 years ago.

From the point of view of family income, it makes little difference whether women are employed part time or full time, all year round or for part of the year only. The in-and-out nature of women's em-

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ployment, insofar as it truly exists, may be due to economic pressure, real or perceived, as much as is the so-called "more stable attachment of men to the labor force." (As has been suggested, one reason for women's changing jobs consists of their attachment to men. The number of men who adjust their occupations to those of their wives is minuscule.) Historically, it has always been true that wives of unemployed men are more likely to work than women with employed husbands, no matter what economic conditions exist. Yet the sharp rise in unemployment which began in 1969 did not lead to any parallel rise in labor force participation by the wives of unemployed men. Married women whose husbands held on to their jobs entered the job market at almost the same rate as did women whose husbands lost their jobs. It is possible, however, that inflation and not unemployment has exerted significant economic pressure.

Economic pressures on women

This alternative poses the following hypothesis. As unemployment increased, the amount of short-time and involuntary part-time employment also rose, while the amount of overtime went down. Due to these factors, family incomes tended to decrease (or not rise) during a period when consumer prices were going steadily upward. The resulting squeeze between family income and outgo prompted wives to enter the labor force in greater numbers than could be predicted from only the figures for unemployment among married men.

If this suggests an expansion of the labor force which reflects not prosperity but inflation, then programs to deal with unemployment by reducing the size of the labor force or by providing new jobs alone will prove insufficient. Economic policy must attack all of the rising costs that beset families but that price controls do not answer. Among the inroads on family income are increased taxes as well as increased prices. Chief among these, especially where both husband and wife are employed, must be reckoned Social Security taxes, which for millions of workers now exceed the amounts withheld from their pay checks for the federal personal income tax. If the overall tax structure becomes steadily more regressive, as it has during the past few years, its impact in a country where more than half the people live in families with incomes below $10,000 will show up more and more in patterns of employment and unemployment, not merely in government budgets of receipts and expenditures.

Women's employment, in the context of economic policy, calls into question the classic prescription that rising unemployment will cre-

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ate pressure against undue increases in wage rates. To the extent that married men have the financial support, *actual or potential,* of working wives, this prediction becomes considerably suspect. The worker who loses his job will not feel the pressure to get work immediately if his unemployment compensation can be supplemented by his wife's earnings. The worker facing a cutback in overtime may be less eager to moonlight if his wife can bring in additional income from part-time work. In fact, over the past two years the duration of unemployment among married men has averaged significantly longer than among single men; and the percentage of married men who have remained unemployed for over 15 weeks has consistently exceeded the figure for single men. Clearly, the family can withstand economic hardship, despite unemployment for the "breadwinner," by means of the earnings of working wives.

On the other hand, the economic pressures on women who have families to support have been intensified by the many months of high unemployment coupled with rising prices. In April 1972, White House spokesmen for the first time abandoned their emphasis on unemployment figures for married men, and began commenting on the rate for all household heads, whether men or women. This figure, while higher than the former one, still understates the hardship for the women involved. For men, wives represent an income-producing resource to the family whether or not they are currently in the labor force: They may have property income, they may be potential earners if conditions change, they may be able to add unpaid labor to a family business or make a given income stretch farther by using more of their unpaid labor at home. But for women who are heads of households, no such additional income-producing resource exists because, by definition, there is no man in the household—not even a disabled husband who could at least babysit while the wife earned income outside the home.

It should also be realized that job opportunities for women are concentrated in lower-paying occupations; a full-time, year-round female worker earns 58 per cent of what a full-time, year-round male worker does. Add to this the difficulties (both in time and money) of maintaining a household for single women with families, and the possibilities of their actually taking advantage of a job opportunity become even more circumscribed. Low-income people probably become skilled at "marginal analysis" fairly quickly, so that the difference between working and becoming eligible for welfare by not working often leads women with families to support to the second choice. Even for those who have been employed, any period

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of unemployment may settle the question in favor of going on welfare. Yet unemployment rates for women who are household heads, while available in a Bureau of Labor Statistics reference publication, have never been widely publicized. From 1969 to 1971, this figure rose from 2.8 to over six per cent; in August of last year it was 5.9 compared to a 2.6 per cent figure for adult men who are household heads and whose families may in fact be partially or largely supported by their wives.

Transitional unemployment

In terms of economic policy, the impact of women's employment on labor force participation by men has not even been analyzed. Instead, it is generally accepted that the female working population is more volatile, with a less stable attachment to the working force, than is the male component. To some extent this view represents a conviction that women generally follow a life pattern which, if it ever existed to any significant degree, disappeared almost as quickly as it was invented: The "typical role" discerned by some analysts and even recommended as a model by some planners found the modern young woman entering the job market when she left school or college and then working for a few years until she married and left either to follow her husband elsewhere or to keep house and raise a family. During the "child-bearing years" she would of course retire from the labor force, and not until her youngest child was grown, or at least safely in high school or college, would she be ready for employment again. At that point, she might take up part-time work, or even a full-time job, depending of course on her husband's career and its demands on her time. This prescription provides a rationale for ignoring the facts about working women and their concerns. The transition between jobs or between job and home could be accepted as sufficient explanation of why women are unemployed, if women's chief occupation is with home and family.

Yet over the past 20 years, labor force participation rates for married women with children have risen faster than for those without children. During the 1960's, the number of working wives with children under six rose from 18 per cent of all such women to almost one third. In 1970 there were eight million working wives with no children, but there were four million with children under six, and six million with older children. (And of course, there were 6.5 million single women, who frequently get overlooked because of

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the prevailing notion that the female role is that of wife and mother.) Despite these facts, the argument that a woman's "life cycle" dictates intermittent periods of employment and dropping out of the labor force has persisted.

In its *Annual Report to the President*, the Council of Economic Advisers referred to this model, or to parts of it, in questioning the usefulness of "general expansive measures" for doing away with transitional unemployment. The Council is quite correct in pointing out that unemployment can arise from "the mobility of the American population, normal market frictions, seasonal variations, and some structural problems." Indeed, our only experience with full employment, during World War II, led to rapid acceptance of the concept of over-full employment where labor was inefficiently used because transitional unemployment was lacking. But the Council then went on to identify transitional unemployment with only two groups, women and teenagers, and to argue that much of the unemployment among women represents the type of transitional unemployment inherent in the life pattern described.

In fact, it may well be that a new type of transitional unemployment among *men*, and particularly among married men, has been created by the increased productivity and earnings of women in the labor force. Because unemployment does not exert as severe economic pressure on the family with a working wife, married men can remain out of work longer than single men. Aside from such quantitative effects, women's employment has also influenced the qualitative aspects of male participation in the labor force. When women themselves are asked why they are working, they frequently answer "to pay for a college education." The phenomenon of the young married woman supporting her husband through graduate school needs no statistical reinforcement, nor does the familiar picture of the woman in her late forties or early fifties, often married to a business executive or a professional, whose wages help meet her children's tuition bills. At middle and lower income levels, women work to help pay for special education or training for their husbands, whether in the form of academic courses or on-the-job training. (In each of these cases, of course, the investment in human capital accrues to the man but is funded by the woman, which raises a provocative question for the economic analysis of property rights.) To some extent, therefore, working women have enabled men to drop in and out of the labor force when any such educational opportunity presents itself or seems reasonable to pursue. Furthermore, if the level of technical achievement and potential

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productivity among men rises as a result of the greater skills acquired through such efforts, presumably their mobility rises also. They are then more likely to shift jobs, both of their own volition and in response to market conditions.

Myths and misconceptions

Finally, one further misconception about women in the labor force has been allowed to persist, with serious implications for economic policy. The Council of Economic Advisers, in explaining transitional unemployment, stated:

> The amount of labor force turnover of adult women appears lower than that of teenagers but greater than that of men. Because of child care and family responsibilities, women are likely to enter and leave the work force more frequently than men, both over the course of a year or a lifetime. By contrast adult males have a very stable attachment to the labor force. About 83 per cent of males 20 years and over are in the labor force (96 per cent of those 25-54 years) and only a trivial proportion are voluntary part-time or part-year workers.

Interestingly, in citing the "stability" of the male labor force the Council does not mention that the 83 per cent participation figure represents a significant decline since 1950. Even within the age 25-54 group, a smaller proportion of men are working or looking for work today than was true in 1950. The overriding impression left by this paragraph is of large numbers of women (as opposed to a "trivial proportion" of men) seeking part-time employment, and whose job turnover rates exceed those of men because they drop in and out of the labor force. If this seems to read too much into one paragraph, an analysis by the Chase Manhattan Bank can substitute. In a report published in June, economists there pointed out that women, who "are less likely than men to be full-time workers with established skills and a record of steady employment . . . traditionally experience higher than average unemployment rates."

Yet as I have pointed out, fewer than one out of five adult women in the labor force worked part time on a *voluntary* basis. This is still higher than the comparable figure for men, but the fact that 80 per cent of the female work force wants to work full time should be emphasized, rather than perpetuating the myth that most women are engaged in part-time work. The only studies of labor turnover among men and women in *comparable jobs* have found that the quit rate for both men and women is less than three out of 100 workers (quarterly basis). The most recent data, collected by the

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Department of Labor in 1968, indeed verify that more women quit than men—but the figures are 2.6 per cent for women and 2.2 per cent for men, which scarcely bears out the implications of the statements quoted above. (The same study found the layoff rate for women to be 2.6 per cent while that for men was 2.2 per cent.) Too often, as a British analyst noted, observers compare the quit rate of managers with that of their secretaries and attribute the difference to one feminine characteristic or another. Finally, a study of occupational mobility shows that men, not women, change jobs more frequently, and here the difference is more significant. Data for 1965 and 1966 show that only seven per cent of the women but 10 per cent of the men had changed occupations. (This reflects, of course, the smaller number of occupations open to women than to men.) The woman born in 1940 can look forward to a work life of well over 20 years, and if the trend of rising employment among young mothers continues, the accepted myth of the female life pattern described earlier may finally be abandoned.

The intellectual task facing economists today is not to use out-of-date sociological models of sex roles but to analyze the separate labor markets that exist for men and women. If there are structural problems that prevent full employment, they consist of trouble spots in the economy, not in the labor force. They are barriers to the employment of *people*, whether they are black or white, male or female, under 20 or over 50. These are the proper areas for investigation and for policy, and if attention could be directed there, instead of to defending present policies or offering persuasive but irrelevant analyses of change, we would all be better off.

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Busing: a Review of "The Evidence"

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DAVID ARMOR's "The Evidence on Busing," (*The Public Interest*, No. 28, Summer 1972) presented a distorted and incomplete review of this politically charged topic. We respect Armor's right to publish his views against "mandatory busing." But we challenge his claim that these views are supported by scientific evidence. A full discussion of our reading of the relevant research would be too lengthy and technical for the non-specialist. We must limit ourselves here to outlining and discussing briefly our principal disagreements with Armor, which center on four major points.

First, his article begins by establishing unrealistically high standards by which to judge the success of school desegregation. "Busing," he claims, works only if it leads—in *one* school year—to increased achievement, aspirations, self-esteem, interracial tolerance, and life opportunities for black children. And "busing" must meet these standards in *all* types of interracial schools; no distinction is made between *merely desegregated* and *genuinely integrated* schools.

This "integration policy model," as it is labeled, is *not* what social scientists who specialize in race relations have been writing about over the past generation. Indeed, Armor's criteria must surely be among the most rigid ever employed for the evaluation of a change program in the history of public education in the United States.

Second, the article presents selected findings from selected studies as "*the* evidence on busing." The bias here is twofold. On the one hand, the few studies mentioned constitute an incomplete list and are selectively negative in results. Unmentioned are at least seven investigations—from busing programs throughout the nation—that meet the methodological criteria for inclusion and report *positive* achievement results for black students. These seven studies are widely known.

On the other hand, only cursory descriptions are provided of the few investigations that are reviewed. Mitigating circumstances surrounding black responses to desegregation are not discussed. For example, we are not told that educational services for the transported black pupils were actually *reduced* with the onset of desegregation in three of the cited cities. In addition, negative findings consistent with

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the paper's anti-busing thesis are emphasized, while positive findings from these same cities are either obscured or simply ignored. Newer studies from three of the cited cities showing more positive results are not discussed.

Positive findings are also obscured by the utilization of an unduly severe standard. The achievement gains of black students in desegregated schools are often compared with white gains, rather than with the achievement of black students in black schools. But such a standard ignores the possibility that *both* racial groups can make more meaningful educational advances in interracial schools. Indeed, this possibility actually occurs in three of the cities mentioned by Armor. Yet he does not inform us of this apparent dual success of desegregation; instead, "busing" is simply rated a failure because the black children did not far outgain the improving white children.

Third, the paper's anti-busing conclusions rest primarily on the findings from one short-term study conducted by Armor himself. This investigation focused on a voluntary busing program in metropolitan Boston called METCO. Yet this study is probably the weakest reported in the paper. Our reexamination of its data finds that it has extremely serious methodological problems.

Two major problems concern deficiencies of the control group. To test the effects of "busing" and school desegregation, a control group should obviously consist exclusively of children who neither are "bused" nor attend desegregated schools. But our check of this critical point reveals that this is not the case. Among the 82 control students used to test the achievement effects of METCO at all 10 grade levels, we obtained records on 55. Only 21 of these 55 actually attended segregated schools in the tested year of 1968-69. Many of the 34 (62 per cent) desegregated children by necessity utilized buses and other forms of transportation to get to school.

Incredible as it sounds, then, Armor compared a group of children who were bused to desegregated schools with another group of children which included many who *also* were bused to desegregated schools. Not surprisingly, then, he found few differences between them. But this complete lack of adequate controls renders his METCO research of no scientific interest in the study of "busing" and school desegregation. Since this METCO investigation furnished the chief "evidence" against "busing," Armor's conclusions are severely challenged by this point alone.

Serious, too, is an enormous non-response rate in the second test administration, a problem alluded to by Armor only in a footnote. For the elementary students, only 51 per cent of the eligible METCO students and 28 per cent of the eligible "control" students took part in both of the achievement test sessions. The achievement results for junior and senior high students are also rendered virtually meaningless by the participation of only 44 per cent of the eligible METCO students and 20 per cent of the eligible "control" students. Compare these percentages to the survey standard of 70 to 80 per cent, and one can appreciate the magnitude of the possible selection bias intro-

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duced into the METCO results by the widespread lack of student participation. Efforts to compensate for these high non-response rates through the use of cross-sectional samples that also suffer from extensive non-response are insufficient.

There are other problems in the METCO study. Some children were included who initially performed as well as the test scoring allowed and therefore could not possibly demonstrate "improvement"; in fact, these pupils comprise one sixth of all the junior high pupils tested for achievement gains in reading. Moreover, the conditions for the third administration of the attitude tests were different for the METCO students and the "controls": The former took the tests at school and the latter took them at home with their parents as proctors. Even apart from the severe control group problems, then, the faulty research design makes any conclusion about differences in racial attitudes between the two groups hazardous.

The inadequate discussion of the METCO study in Armor's article makes it virtually impossible for even the discerning reader to evaluate it properly. We uncovered its many errors only from unpublished earlier materials and from reanalyzing the data ourselves. The METCO discussion is inadequate in other ways. Differential statistical standards are employed, with less rigorous standards applied to findings congruent with the article's anti-busing thesis; attitude differences among METCO schools are not shown; and misleading claims of consistency with other research findings are made.

From this assortment of "evidence," Armor concludes authoritatively that "busing" fails on four out of five counts. It does not lead, he argues, to improved achievement, grades, aspirations, and racial attitudes for black children; yet, despite these failures, he admits that desegregated schools do seem somehow to lead more often to college enrollment for black students.

The picture is considerably more positive, as well as more complex, than Armor paints it. For example, when specified school conditions are attained, research has repeatedly indicated that desegregated schools improve the academic performance of black pupils. Other research has demonstrated that rigidly high and unrealistic aspirations actually deter learning; thus, a slight lowering of such aspirations by school desegregation can lead to better achievement and cannot be regarded as a failure of "busing." Moreover, "militancy" and "black consciousness and solidarity" are not negative characteristics, as Armor's article asserts, and their alleged development in desegregated schools could well be regarded as a further success, not a failure, of "busing." Finally, the evidence that desegregated education sharply expands the life opportunities of black children is more extensive than he has indicated.

Consequently, Armor's sweeping policy conclusion against "mandatory busing" is neither substantiated nor warranted. Not only does it rely upon impaired and incomplete "evidence," but in a real sense his paper is not about "busing" at all, much less "mandatory busing." Three of the cities discussed—among them Boston, the subject of Armor's own research—had *voluntary,* not "mandatory busing."

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"Busing" was never cited as an independent variable, and many of the desegregation studies discussed involved some children who were not bused to reach their interracial schools. Indeed, in Armor's own investigation of METCO, some of the METCO children were not bused while many of the controls were.

Fourth, objections must be raised to the basic assumptions about racial change that undergird the entire article. Public school desegregation is regarded as largely a technical matter, a matter for social scientists more than for the courts. Emphasis is placed solely on the adaptive abilities of black children rather than on their constitutional rights. Moreover, the whole national context of individual and institutional racism is conveniently ignored, and interracial contact under any conditions is assumed to be "integration."

Now we wish to pursue these basic points in more detail.

I

Unrealistic standards for judging the effects of "busing." The article advances an "integration policy model" which it claims grew out of social science and guided "the integration movement." The model allegedly maintained that *all* school desegregation would result in improved black achievement, aspirations, self-esteem, racial attitudes, and educational and occupational opportunities (Armor, p. 96). This interpretation of "the integration policy model" is at sharp variance with what specialists in this field have been writing over the past generation.[1] The fundamental premise of social scientists over these years was that racial segregation as it is typically imposed in the United States leads directly to a multitude of negative effects not only for black America but for the nation at large. (The evidence for this premise is extensive, and Armor does not contest the premise.) But social scientists have not made the error of contending that because enforced racial segregation has negative effects, *all* racial desegregation will have positive effects. It requires little imagination to think of hostile conditions of school desegregation that would limit its benefits for both races.

At the heart of this misconception is a persistent misreading of Gordon Allport's (1954) theory of intergroup contact. Armor cites a quotation from Allport delineating the crucial conditions that he held to be essential before positive effects could be expected from intergroup contact: equal status, common goals, institutional supports, and a non-competitive atmosphere that is likely to lead to "the perception of common interests and common humanity." Yet Armor summarizes this quotation by stating: "The clear key to breaking the vicious circle, then, was contact." This is *not* what Allport wrote; the key, Allport argued, is contact *under particular conditions.*

Later in his article Armor adds a brief discussion of one of these conditions—equal status between the two groups. Allport and other contact theorists have maintained that this condition is met by equal status, dignity, and access to resources *within* the contact situation

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itself (e.g., Pettigrew, 1971). Armor reinterprets this condition so that it is met only if the two groups bring equal societal status *to* the situation, a rigorous test indeed in a society where racial discrimination has long been endemic. We know of no relevant contact research that supports this reinterpretation of the theory, and vague references to conflict in Northern Ireland and the Middle East hardly suffice as evidence. But armed with his own reinterpretation, Armor (p. 111) writes: "Therefore, we have to question whether integration programs for black and white children can ever fulfill the equal status condition as long as socio-economic and academic inequalities are not eliminated." Here the misreading of Allport's contact theory is fashioned into not only an explanation of presumed "negative" results from interracial schools but a not-so-subtle rationale for at best gradualism and at worst a return to racially segregated education throughout the nation.

The basic weakness, then, in this description of an "integration policy model" is that it assumes positive results for *all* interracial schools rather than for just those meeting the conditions for optimal contact. This erroneous assumption is best illustrated by reference to the chief policy document relied upon by Armor: *Racial Isolation in the Public Schools,* issued by the U.S. Commission on Civil Rights (1967). The quotation Armor cites from this report emphasizes the harmful effects of racially isolated schooling, and it does not specify all of the five hypotheses which he somehow deduces from it. That the Commission clearly understood that interracial schools in and of themselves are not necessarily effective schools is demonstrated by the following passage which was not quoted:

> Whether school desegregation is effective depends on a number of factors. These include the leadership given by State and local officials; the application of the plan to all schools in the community; the measures taken to minimize the possibility of racial friction in the newly desegregated schools; the maintenance or improvement of educational standards; the desegregation of classes within the schools as well as the schools themselves, and the availability of supportive services for individual students who lag in achievement.

The Commission Report discusses these factors in detail for over eight pages, factors neither mentioned nor measured by Armor. "The integration policy model," then, sets up unrealistic standards for judging the effects of "busing" by ignoring the conditions specified by the two principal sources cited. Its five criteria for success constitute a "straw man," far exceeding the standards applied for the evaluation of other educational programs.

The critical distinction between desegregation and integration is ignored. The racial desegregation of schools is not a static but a complex, dynamic process. To evaluate it fairly, the critical conditions under which it takes place must be assessed. For this purpose, it is important to distinguish between desegregation and integration. Desegregation is achieved by simply ending segregation and bring-

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ing blacks and whites together; it implies nothing about the quality of the interracial interaction. Integration involves Allport's four conditions for positive intergroup contact, cross-racial acceptance, and equal dignity and access to resources for both racial groups.

The neglect of this distinction besets not only Armor's theoretical contentions but his empirical ones as well. No effort is made to look inside of the schools at the *process* of desegregation. The cursory descriptions of the "busing" investigations tell virtually nothing about the conditions of interracial contact that prevailed. (Indeed, a few of the initial reports of these studies failed to describe contact conditions.) For example, we should have been informed by Armor that transported black children in some Riverside schools arrive and leave earlier than the untransported white children and that they have separate reading classes—hardly practices likely to generate interracial contact and lead to integration (Singer, 1972). And we might have been told that minority students in Riverside who were most likely to be in interracial classrooms (high-ability students) performed far better after desegregation than before (Purl, 1971).

In fact, in his Detroit deposition for school segregation, Armor admitted that he had no measures or knowledge in his own study of the METCO schools of such crucial factors as teacher expectations and preparation, the racial composition of the faculties, ability tracking practices, and curriculum changes. A review of "the evidence on busing" is misleading at best without consideration of these indicators of the desegregation versus integration distinction.

II

A biased and incomplete selection of studies. Armor's article makes no attempt to review all of the available evidence on "busing," as its title implies. Instead, the reader is told about only a small number of studies, selected with an apparent bias toward those reporting few positive effects. One hint of this selection is found in Armor's footnote 1, where we learn that he arbitrarily excludes the entire southern United States from his purview, though this severe restriction is not indicated either in his title or his conclusions against "mandatory busing." This unexplained exclusion seems unwarranted, for the bulk of court-ordered "mandatory busing" has occurred in the South.

Armor omits at least *seven* key desegregation investigations—only one of which is from the South—that reach conclusions in conflict with those of his paper. All seven of these desegregation programs involved "busing," and all seven of the studies meet the paper's two stated criteria for inclusion—longitudinal data and an adequate control group. Table 1 summarizes these neglected research reports. Though five of them spanned only one school year, all seven reach *positive* conclusions concerning the effects of school desegregation upon the academic performance of black children. Moreover, none of them found that the process lowered white academic performance. No matter how Armor might wish to view these studies in

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TABLE 1. *Seven Neglected Desegregation Investigations*

STUDY		DESIGN				ACHIEVEMENT RESULTS	
PLACE	AUTHOR(S)	GRADE LEVEL	TYPE OF COMPARISON	CONTROL VARIABLES	TIME OF DESEGREGATION	FOR BLACK CHILDREN	FOR WHITE CHILDREN (IF TESTED)
SOUTHERN DESEGREGATION							
Goldsboro, N.C.	King & Mayer (1971)[1] McCullough (1972)	7-11 cohort	White students and trend during segregation	Convergence curves for regression to mean effects and pre-desegregation trends	2 years	Statistically significant gains in reading closing part of black/white differential; gains in math scores do not close racial gap; gains greatest for initially high achievers	Both reading and math gains; gains greatest for high achievers
SUBURBAN BUSING PROGRAMS							
Newark-Verona, N.J.	Zdep & Joyce (1967)	1-2	Comparable non-transfers	——	1 year	Statistically significantly greater total achievement gains for desegrated in both grades	No negative effects (only difference favors the desegregated)
Rochester-West Irondequoit, N.Y.	Rock *et al.* (1968)	K-2	Comparable non-transfers	Teachers' ratings of ability	3 years	Statistically significantly greater verbal, reading, and math achievement gains on 13 of 27 comparisons for desegregated; no significant differences on remaining 14 comparisons	No negative effects (only differences favor the desegregated)

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TABLE 1. *Continued*

STUDY		DESIGN				ACHIEVEMENT RESULTS	
PLACE	AUTHOR(S)	GRADE LEVEL	TYPE OF COMPARISON	CONTROL VARIABLES	TIME OF DESEGREGATION	FOR BLACK CHILDREN	FOR WHITE CHILDREN (IF TESTED)
NORTHERN CENTRAL CITY DESEGREGATION							
Buffalo, N.Y.	Banks & DiPasquale (1969)	5-7	Comparable non-transfers	—	1 year	2½ months greater achievement gain for the desegregated	No negative effects
New York, N.Y.	Slone (1968)	4	Comparable non-transfers	—	1 year	Statistically significantly greater math achievement gains, and somewhat greater reading gains ($p<.10$), for desegregated	No negative effects
Philadelphia, Pa.	Laird & Weeks (1966)	4-6	Comparable non-transfers	I.Q., grade and sex	1 year	Statistically significantly greater reading, and somewhat greater math, achievement gains for desegregated in fourth and fifth grades	—
Sacramento, Cal.	Morrison & Stivers (1971)	2-6	Comparable non-transfers	—	1 year	Statistically significantly greater gains on three of ten comparisons (5 classes on 2 tests) and greater gains on 6 more, for desegregated	—

[1] Similar results for a cohort of second through fifth grade students have also been obtained in Goldsboro. After two years of desegregated education the *standardized* verbal and mathematical computation achievement scores of both the black and white students had risen. The verbal gains, though not the mathematical computation gains, closed the racial differential slightly. Robert R. Mayer, University of North Carolina at Chapel Hill, personal communication.

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retrospect, there was no reason for their omission in a paper that claimed to present "*the* evidence on busing."

Space limitations prevent a discussion here of these neglected investigations, but five points should be made about them. First, a number of them share methodological problems with the studies that Armor did choose to discuss. Indeed, reviewers of this research literature have uniformly found it methodologically weak (Matthai, 1968; O'Reilly, 1970; St. John, 1970; Weinberg, 1968). Second, these seven by no means exhaust the relevant research literature that meets the paper's dual criteria for inclusion. There are studies on desegregation without busing that reveal positive achievement effects (e.g., Anderson, 1966; Fortenberry, 1959; Frary and Goolsby, 1970). There are a few others that were also left out that found no significant achievement gains associated with desegregation (e.g., Fox, 1966, 1967, 1968). From the perspective of the desegregation versus integration distinction, this mixed picture is precisely what one would expect. Third, these seven studies are not obscure reports; all but the more recent Goldsboro and Sacramento studies are cited in one or more of the standard reviews available on the topic (Matthai, 1968; O'Reilly, 1970; St. John, 1970; Weinberg, 1968).

Fourth, the positive achievement effects revealed by these studies are often not just statistically significant (Armor's criterion) but, more important, are educationally significant as well. The study from Buffalo by Banks and DiPasquale (1969), for example, found a 2.5 month achievement advantage for the desegregated children. Over a 12-year school career, were such an advantage to be replicated each year, this would constitute 2.5 extra years of achievement —a critical addition that could mean the difference between functional illiteracy and marketable skills. Finally, these seven studies do not measure the "pure" effects of desegregation any more than those cited by Armor. Probably there are no instances of school desegregation that are not confounded with curriculum changes, school quality, and other educational alterations. But our point is made: The few studies mentioned in Armor's article constitute an incomplete list and are selectively negative in results.

Biased and incomplete descriptions are provided of the few studies discussed. The cursory reviews of the few studies that Armor did select for attention allow only biased and incomplete descriptions. Since his article never probes the process going on inside the schools, it repeatedly omits mitigating circumstances surrounding black responses to desegregation. For example, no mention is made of the fact that educational services for the transported black students in Ann Arbor, Riverside, and Berkeley were actually *reduced* with the onset of desegregation (Carrigan, 1969; Frelow, 1971; and Purl, 1971). Nor is there any indication that Riverside initially placed many of its bused minority children in the same classrooms, and often with low-achieving white children (Henrick, 1968). No "integration model," not even the new one devised by Armor, is fairly tested under such conditions.

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Moreover, the positive findings that favor desegregation in these studies are often obscured or simply ignored by Armor. In the case of Hartford, for instance, only Wechsler I.Q. data are cited, while extensive results from the Primary Mental Abilities Test and measures of school achievement go undiscussed. When all three types of tests are considered together, a clear pattern of larger gains for the transported children emerges for all four grades from kindergarten through the third grade (Mahan, 1968). Likewise, black pupils in Ann Arbor attained a substantially higher mean I.Q. after one year of desegregation, but this fact is lost from sight by the use of a white comparison. A range of interesting results from Riverside is also omitted. Purl (1971) found that: (a) Bused students who were more dispersed in the classes of their receiving schools outperformed those who—through ability grouping or other means—were clustered in near-segregation style. (b) While the mean achievement of minority pupils with low initial ability scores declined relative to grade level, the achievement of minority pupils with high initial ability scores rose in the desegregated schools. (c) Minority children transported to schools characterized by higher achievement of the receiving white students gain significantly more than comparable minority children transported to schools characterized by low achievement, an effect not linked to the social class levels of the receiving students. (d) The one group of bused minority students who began their schooling in interracial schools achieved better than those who had first experienced segregated education.

The incomplete descriptions also fail to reveal major methodological weaknesses in these cited studies. The Berkeley (1971a) investigation, as a case in point, utilized different tests for comparison over time, precisely the same defect for which an investigation in Rochester (1971) showing a number of positive results is rejected without discussion. The White Plains (1967) investigation employs inadequate control groups drawn from earlier time periods, a faulty procedure that confounds the effects of events over time with those of desegregation.[2] Indeed, the negative conclusions of a follow-up study in Ann Arbor are given without recording the fact that it failed to meet either of the criteria purportedly used for inclusion, for it had no control group whatsoever nor did it gather longitudinal data on the same test (Aberdeen, 1969; Carrigan, 1969).

Finally, several newer reports on these same cities that present results favorable to desegregation are not utilized. Mahan and Mahan (1971), for example, provide more refined analyses on the Hartford achievement data. Pooling the first, third, and fifth grades,[3] they show that the desegregated children in Project Concern do significantly better after two years than their comparable segregated controls on the Wechsler I.Q. and on both the verbal and quantitative scores of the Primary Mental Abilities Test.

Though he cited a Master's thesis on New Haven desegregation, Armor failed to cite a better-known doctoral dissertation on the same city.[4] Samuels (1971) studied 138 black students who had all attended inner-city kindergartens in 1969 and then were assigned

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randomly to one of three conditions: bused into suburban schools, received intensive compensatory education in New Haven schools, or attended regular New Haven schools. After two years, Samuels found that the bused children possessed significantly higher reading scores than the two control groups as well as higher word knowledge scores that approach statistical significance ($p<.07$). Their self-image scores were slightly higher, but not significantly different. Comparisons on word analysis and mathematics yielded no significant differences.

In Berkeley, Frelow (1971) studied the third and fourth grade achievement of poor children, most of them black, over a six-year period that witnessed rapid changes in the city's schools. Though this design, like that used in White Plains, lacks contemporaneous controls, he found that achievement scores rose significantly after the introduction of compensatory programs and went slightly higher still after desegregation despite a reduction in services. Frelow concludes that "when gains are measured against level of instructional services, desegregation produces the most prominent achievement results."

The use of white control groups is inadequate and often misleading. The contention that black children will learn more in integrated than in segregated schools is not tested when black data are compared with those of white control groups. Moreover, the use of a desegregated white control group ignores the possibility that *both* whites and blacks could benefit significantly from integration without "the racial gap" in achievement closing at all. As a matter of fact, precisely this possibility occurs in Riverside, Berkeley, and Ann Arbor—though this is not mentioned by Armor and is allowed to mask black gains in desegregated schools.

For Riverside, Armor reports that even for the fourth-grade group that had been desegregated since kindergarten "the minority/white gap had not diminished. . . ." But actually the white test scores being used for a comparison had improved after desegregation relative to national norms (Purl, 1971). Thus, the fact that the minority students held the "gap" constant represents improvement; this is indicated, too, by these minority students' relative gains in grade equivalents.

For Berkeley, Armor reports in a footnote that "black achievement is as far behind (or *further* behind) white achievement after two years of integration as before integration." But *both* white and black grade equivalents in grades one, two, and three went up across age cohorts after two years of desegregation; yet since they rose in virtually equal amounts, the "black/white gap" was not narrowed (Berkeley, 1971a, 1971b). The measure here is grade equivalents, not percentiles. Thus, keeping "the racial gap" from expanding is an accomplishment in itself for desegregation, since the typical result of segregated schools is an ever-widening "racial gap" in grade equivalents (Coleman *et al.*, 1966; Mosteller and Moynihan, 1972).

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The most extreme case of this misleading use of white controls, however, occurs for Ann Arbor (Carrigan, 1969). Here the bused black students were "a multi-problem group" with a greater incidence of "general health problems" and behavioral "problems requiring special professional help." Yet they gained an average of 3.86 I.Q. points during their first year of desegregation. They were compared with generally high-status white children, many of whom came from academic families, who gained an average of 4.28 I.Q. points. "Busing" failed, in Armor's terms, because "the racial gap" did not close. But can a program which utilizes fewer services with a multi-problem group of youngsters, and yet is associated with a nearly four-point average increase in I.Q. during one school year, be unquestionably ruled a failure? We think not, even if these "bused" pupils did not gain more than high-achieving white youngsters from a university community.

This point represents a crucial difference between our perspective and Armor's. We believe it to be unrealistic to expect any type of educational innovation to close most of the racial differential in achievement while gross racial disparities, especially economic ones, remain in American society. Furthermore, we know of no social scientists who ever claimed school desegregation alone could close most of the differential. We are pleased to note the many instances where effective desegregation has apparently benefited the achievement of both black and white children, and where over a period of years it appears to close approximately a fourth of the differential.

But to insist that "mandatory busing" must close most of the achievement differential by itself in a short time or be abolished is, to understate the case, an extreme position. Indeed, Armor himself has wavered on this point. In *The Public Interest* he wrote: "The ideal control group, of course, would consist of black students who are identical to the integrated students in every way except for the integrated experience" (Armor, p. 97), though white students in the same school constituted an "adequate" control. Later, however, while testifying in support of anti-busing legislation before the Senate Subcommittee on Education, he used white pupils as the critical comparison. This stern criterion leads to some strange conclusions. A desegregation program that dramatically raises the achievement levels of both racial groups is judged a failure when it does not close most of the racial disparity, but another desegregation program that entirely closes the gap by raising the blacks' scores and *lowering* the whites' scores would have to be deemed a success!

III

Serious weaknesses in the METCO research. Armor's article relies most heavily upon his own research on Boston's suburban program known as METCO. Far greater space—including a dozen graphs—is devoted to the METCO research than to all of the other research combined; and the METCO work is the only investigation that is

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relied upon for support of all five of the conclusions concerning the effects of "busing." Yet a careful reanalysis of these METCO data reveals a host of serious weaknesses that center on five concerns: (a) the unrepresentativeness of the METCO program, and problems regarding (b) the control group, (c) the sample, (d) test administration, and (e) the analysis.

a. *Unrepresentativeness of METCO program.* Not only is "busing" not "mandatory" in METCO, but the program is highly atypical of desegregation efforts with "busing" around the nation. METCO is a voluntary program, and it has disproportionately attracted middle-class black students. This class bias may help explain why METCO children in the first year of the program attained a higher average I.Q. than the white national average (Archibald, 1967) and why in Figures 1 and 2 of Armor's article all 10 grade levels show relatively high achievement scores. Moreover, METCO children comprise only a minute fraction of their student bodies, with less than four per cent in any one school in 1969. Black faculty are rare in virtually all of the METCO schools. Indeed, some METCO schools have had all-white staffs, and until recently even all of the bus drivers were white. Thus, given METCO's "tokenism" in students and staff, as well as its social class bias, direct generalizations from this program to "busing" throughout the United States appear dubious at best.

b. *Control group problems.* The most serious weakness of the METCO research involves the students who were employed as "controls." The study's design obviously requires that none of these control students were either desegregated or "bused." But a careful review of the available records reveals that this essential condition is not met.[5] Among the 41 "control" youngsters at the elementary level, we obtained records on 17. Only seven of these 17 pupils were actually attending segregated schools during 1968-69, while 10 (59 per cent) were attending desegregated schools. Similarly, among the 38 (out of a total of 41) "controls" at the junior and senior high levels whose records we obtained, only 14 were in segregated schools during the tested year, while 24 (63 per cent) were attending desegregated schools.

All told, then, of the 55 students whose records were secured, 34 (62 per cent) actually went to desegregated schools and many of them used buses and other means of transportation.[6] Even if we assume that all 27 students whose records were unavailable went to segregated schools (an unlikely possibility), these data still mean that at least 41 per cent (34/82) of the "control" students were in fact experiencing a racially desegregated education. Indeed, these desegregated "controls" were generally in schools with a greater interracial mixture than those attended by the METCO children.

This failure of the METCO study to have an adequate control group cannot be overemphasized. It means that *all* of the METCO comparisons between the METCO and "control" children in Armor's article are not valid indications of any differences attributable to "busing" or school desegregation. For such comparisons may also

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reflect the different effects of suburban versus inner-city desegregation and token versus substantial desegregation. In short, we believe this weakness alone eliminates the METCO study from being relevant to "the evidence on busing," and makes our further criticisms of the study almost superfluous.

Other problems involve the use of siblings of METCO students as "controls." "This design feature by no means guarantees the equating of the groups," wrote Herbert Walberg (1969) in the initial write-up of this investigation, "since there may be bias in the family's choice of the child to be bused. . . ." Indeed, there is potential bias in the selection by families, but the direction is not clear. The academically superior child might be chosen more often by his parents; or, as METCO officials suspect, the child having difficulties in Boston's schools might be chosen more often. Moreover, the use of siblings for controls tends to confound sex, grade level, and age with family climate and social class.

c. *Sample problems.* The METCO research suffers, too, from both small numbers and a severe loss of eligible subjects. Limited sample size makes finding statistically significant differences in achievement between the experimental and "control" groups less likely; or, put differently, small sample sizes aid in supporting the anti-desegregation thesis of the article. The extent of this problem is shown in Table 2, which provides the sample sizes by grade level. The question arises as to how large the METCO group dif-

TABLE 2. *METCO Sample Sizes by Grade Level and Type of School*

GRADE LEVEL	METCO[1]	"CONTROL"	TYPE OF SCHOOL ATTENDED BY "CONTROLS"		
			SEGREGATED	DESEGREGATED	UNAVAILABLE
3rd & 4th	88	14	2	3	9
5th & 6th	59	27	5	7	15
Elementary School Totals	**147**	**41**	**7**	**10**	**24**
7th	47	11	6	5	0
8th	31	10	4	5	1
9th	47	6	1	4	1
Junior High School Totals	**125**	**27**	**11**	**14**	**2**
10th	53	4	0	3	1
11th	18	8	3	5	0
12th	1	2	0	2	0
Senior High School Totals	**72**	**14**	**3**	**10**	**1**

[1] These data are taken from our reconstructed data tapes. Armor lists 123 junior high METCO students in his Figure 2, but he inadvertently dropped two cases.

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ferences in achievement would have had to be before the sample sizes employed could have detected a statistically significant difference even at the .05 level of confidence? By our calculation, the answer at the junior high level, for example, is that the METCO students would have had to gain at least 0.4 of a grade *more* in average achievement on the test norms than the "control" group.[7] This is an unrealistic expectation over a duration of only *seven* months, especially for comparisons among children who are close to grade level. An educationally meaningful average gain difference over such a short period would have been 0.2 of a grade more for the METCO students. But this would have required sample sizes of roughly 200 in each group to have reached statistical significance for a two-tailed test. Instead, only 125 METCO and 27 "control" junior high students were tested. The same point can be made about the other grade levels. We conclude, therefore, that the criterion of statistical significance was inappropriate for evaluating the METCO program when the sample sizes were so small.

The loss of subjects occurred in two stages. Among the elementary students, in the first test administration in October 1968, there was a 23 per cent loss of eligible METCO students and a 35 per cent loss of eligible "control" students.[8] In the second test administration in May 1969, 34 per cent of the METCO and 56 per cent of the "control" students who had taken the tests seven months earlier did not retake them. Combined, then, the achievement results on these students included only 51 per cent of the eligible METCO and 28 per cent of the eligible "control" participants. The situation was even worse for the junior and senior high students, whose achievement results were based on only 44 per cent of the eligible METCO and only 20 per cent of the eligible "control" participants. Furthermore, only eight per cent of the "controls" took part in all three test administrations.

Contrast these percentages with Useem's (1971, 1972) response rate of 87 per cent in her study of white students in METCO schools. Compare them, too, with the accepted survey research standard of at least a 70 to 80 per cent response rate, and one can appreciate the high level of potential bias introduced by this loss of subjects from Armor's study. An attempt to compensate for these impaired data by utilizing cross-sectional results is not an adequate remedy for many reasons, some of which are provided by Armor himself when he condemns cross-sectional investigations. Besides, there was a considerable loss of eligible subjects, and thus potential bias, in the cross-sectional data as well.

d. *Test administration problems.* "The control group," Armor argued in his Detroit deposition for school segregation, "has to be measured in the same way that the treated group is." He further maintained that "we must measure them before the treatment, and put one through the treatment and one not, to assess the effect of a program." We agree, but his METCO research failed on both counts.

The third testing in May 1970, which involved attitudes but not achievement, took place under markedly contrasting conditions for

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the experimental and control groups. While the METCO children answered the questions in school, the control children answered them at home through a mailed questionnaire that explicitly requested the parents to serve as proctors. This procedure risks two related sources of bias. A wealth of research has demonstrated how different situations can lead to sharply different responses; and the home administration of the controls' testing opens the possibility for family members to influence the answers directly.

Armor expresses amazement that the METCO children revealed as a group more militant and ideological responses than the "control" children, but the differential testing administrations provide a possible explanation. Repeated surveys indicate that young black peers at school are far more likely to be militant and ideological than older parents at home (Campbell and Schuman, 1968; Goldman, 1970); and research in social psychology has shown that such different situational influences can have a sharp effect on group-linked attitudes (Charters and Newcomb, 1952).

On the second point, measuring the groups *before* the treatment, the METCO research also fails. The METCO pupils were measured initially in October 1968, *after* all of them had begun for a month or more their year in the METCO school. Moreover, 45 per cent of the METCO children were not beginning "the treatment" of suburban education, for they had already been in the program for either one or two years.

Finally, studies utilizing achievement tests require well-motivated students who are trying to do their best. We learn from those in attendance at both the first and second test administrations, however, that motivation was apparently not high. And no wonder. The students, METCO and control, had no special incentive for taking the lengthy tests on a holiday in a Boston technical school described by Walberg (1969) as "an old, run-down, ill-cared-for building." This low level of motivation probably accounts for the small turnout for the second test.

e. *Analysis problems.* Even if there were no serious control group and sample problems, numerous data errors place Armor's analysis of the METCO results in serious question. One child was included who apparently did not take the verbal test initially at all; his post-test scores were then treated as a total gain from a base of zero. A sixth (25 of 151) of the junior high students initially scored virtually as high as the achievement test scoring allowed. Thus, this "ceiling effect" made it impossible for their post-test scores to advance, and their performance was treated as showing "no gain." Such problems, together with clerical errors, help explain why such talented children are shown to make such slight achievement gains in Armor's Figures 1 and 2. But given the irreparable control group and sampling problems, no purpose is served by a reanalysis of these data that corrects for these errors of analysis and data handling.

Inadequate discussion of the METCO research. The reader is not told enough in Armor's article to evaluate the METCO research fully.

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Most of our critical comments are based on information gleaned from a reanalysis of the raw data, the examination of unpublished papers on the research (Archibald, 1967; Walberg, 1969; and Armor and Genova, 1970), and a review of Armor's court testimony concerning the research. The discussion of the METCO work is also inadequate in other ways: (a) Differential statistical standards are employed; (b) attitude differences between METCO schools are not shown; and (c) misleading claims of consistency with other research findings are advanced.

a. *Differential statistical standards.* Rigid standards of statistical significance are uniformly applied to findings that favor school desegregation. Findings of positive effects in other studies that approach statistical significance are summarily dismissed as "not significant." But these standards are relaxed considerably when findings interpreted as negative to school desegregation are discussed. For instance, Figure 3 is provided to show how the grades of METCO's junior and senior high school pupils declined slightly, and this finding is emphasized in the conclusions (Armor, p. 109). Yet there is no significant difference between the METCO and the control groups on changes in grades. Similarly, a slightly greater increase among METCO students in wanting a school with no more than half-white student bodies is emphasized (Armor, pp. 102-103). Though ". . . the differential change is not statistically significant," Figure 7 is devoted to it. And later in the conclusions, this finding is utilized without qualification as part of the evidence that "bused" black students have become more supportive of "black separatism."

b. *Attitude differences between METCO schools are not shown.* Armor's article assumes that the METCO program consisted of the same "treatment" for all of the children participating in it. Consequently, attitude differences across METCO schools were not shown; nor, as noted earlier, were any variables utilized to take into account what type of educational programs were actually occurring inside the various METCO schools.

Actually, of course, there are as many different METCO programs under way as there are separate METCO schools. But consider the contrasting policy implications of providing only the total results as opposed to school-by-school results. Suppose a particular school program aimed at improving racial attitudes were attempted in eight schools, and that the overall effect was minimal. The policy implication would be to regard the program a disappointment and to consider abandoning it. Suppose further that a meaningful effect had in fact been registered in all but two schools, but that attitudes in these two were so unfavorable that they virtually obscured the favorable attitudes of the other six in the total data. Now the policy implication *from the same data* would be to regard the program as encouraging and to find out how to change the deviant two to make them more like the successful six schools. In short, the variability across schools is a critical consideration in judging a program.

Our Figure 1, from Useem (1971), shows that a situation similar to this existed for the METCO program in 1969. Note that schools F and

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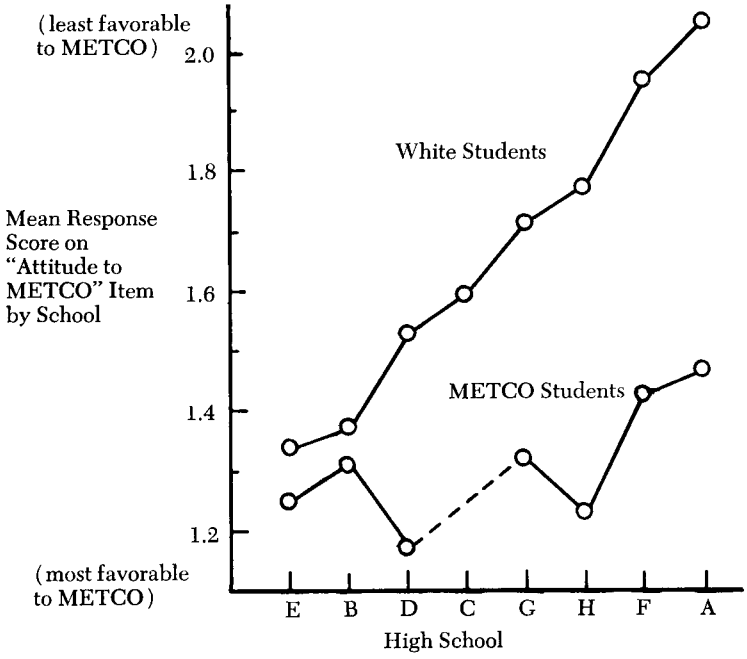
FIGURE 1. *Attitudes of METCO and White Students Toward the METCO Program by High School*[1]



1 Data from METCO students in School C were not available. The Figure is taken from Useem (1971).

A evince by far the most anti-METCO sentiment among both white and black pupils. Note, too, that black attitudes toward METCO are consistently more favorable than those of whites, though there is a positive relationship across schools in the attitudes of the two groups. With such wide differences between METCO schools, how can a simple judgment of success or failure be passed upon the entire program?

c. *Misleading claims of consistency with other research findings are advanced.* Two studies are cited as providing supporting evidence for the METCO results; but their descriptions are so incomplete as to be highly misleading. Useem's (1971, 1972) METCO investigation is given as evidence for how interracial contact in METCO schools leads to worse race relations. Her complete findings, however, point to a different conclusion, and we shall return to these findings shortly. The other citation refers to Armor's earlier reanalysis of the Coleman report data:

> An extensive reanalysis of the Coleman data showed that even without controlling for social class factors, "naturally" integrated (i.e., non-bused) black sixth-grade groups were still one and one-half standard deviations behind white groups in the same schools, compared to a national gap of two standard deviations. This means that, assuming the Coleman data to be correct, the *best* that integration could do would be to move the average black group from the 2nd percentile to

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> the 7th percentile (on the *white* scale, where the average white group is at the 50th percentile). (Armor, p. 100)

Such a statement is extremely misleading, and it requires clarification. It appears to assert that there is some upper limit on the possible achievement gains through "busing" of blacks relative to whites. No such assertion is possible. Moreover, the evidence for this claim is based on data from *groups* of children who are in general not bused and for whom there are only Coleman's cross-sectional data. The statement, then, implies a causal relation from cross-sectional data, a practice correctly condemned earlier by Armor. The statement further implies that there is some intrinsic, if unspecified, connection between the gains possible from "busing" and the inferred gains estimated from cross-sectional data.

More misleading still is the use of *group* percentiles. Technically, it may be correct that the average black *group* mean in desegregated sixth grades is only at the 7th percentile when compared with the means of white *groups*. But the obvious misinterpretation that can easily arise is that the average individual black *student* in a desegregated school is only at the 7th percentile compared with the individual white student norms. Such an interpretation is patently *wrong*. Though Armor can argue that his statement is technically accurate, we feel that he has an obligation to inform the lay reader fully so that such a misinterpretation could not occur.

The misleading statement utilizes standard deviations based on group means rather than on individual scores. Group standard deviations are invariably smaller than standard deviations based on the individuals within the groups. Instead of the average black *group* in desegregated sixth grades being at the *7th* percentile of white *group* norms, then, we estimate that the average black *individual* in desegregated sixth grades ranks between the *25th* and *30th* percentiles of white *individual* norms.[9] Indeed, Figure 2 of Armor's article shows that the black senior high students in the METCO research average between the 25th and 43rd percentiles in individual reading achievement.

The achievement effects of "busing" are more complex and positive than reported. Armor concludes that "busing" fails on four of the five standards he alone sets for it. One of these alleged failures concerns the academic achievement of black students. From the selected findings of selected studies, Armor concludes that desegregation research throughout the nation has typically found no statistically significant enhancement of black achievement. Further, he claims that the METCO results support this conclusion. But we have noted how this conclusion was reached through the omission of at least seven busing investigations with positive black achievement results and through serious weaknesses in the METCO research.

This is not the place for a complete review of the relevant research literature. But our evaluation of the available evidence points to a more encouraging, if more tentative and complex set of conclusions.

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First, the academic achievement of both white and black children is not lowered by the types of racial desegregation so far studied. Second, the achievement of white and especially of black children in desegregated schools is generally higher when some of the following critical conditions are met: equal racial access to the school's resources; *classroom*—not just school—desegregation (McPartland, 1968); the initiation of desegregation in the early grades; interracial staffs;[10] substantial rather than token student desegregation (Jencks and Brown, 1972); the maintenance of or increase in school services and remedial training; and the avoidance of strict ability grouping.

Grading changes before and after desegregation are meaningless if differential grading practices are not considered. "Busing" also fails, according to Armor, because the grade average of the METCO students in junior and senior high schools declined. The average METCO grade decline is slight (—0.12 on a four-point scale), although he described it as "considerable" (Armor, p. 109). Nor is the difference in grade changes between the METCO and control groups statistically significant. Moreover, the greater drop in METCO grades than in control grades may be an artifact of the enormous non-response rate discussed earlier, for the full cross-sectional data show the controls' grades falling as much as those of the METCO children (—0.14 to —0.13).

Black grades also fell after desegregation in Evanston, we are informed in Armor's footnote 4. But we are not informed that the same study shows that white grades also fell and that there were no significant differences "in the frequencies of earned grades within each group" (Hsia, 1971). By contrast, when black pupils left a segregated junior high school in Sacramento in 1964, they soon received higher grades in the desegregated schools and maintained this improvement throughout their junior high years (Morrison and Stivers, 1971). However, none of these results are convincing, since differential grading practices are not controlled.

Shifts in aspirations and "academic self-image" during desegregation are positive in meaning. Armor further contends that "busing" fails because it lowers both the aspirations and academic self-concept of black children. Several qualifications are briefly discussed initially (Armor, pp. 101-102), but when the conclusions are drawn, this METCO "finding" has become unqualifiedly one of the four failures of "busing" (Armor, p. 109).

Actually, the METCO data on the subject are by no means clear. Two of Armor's three relevant Figures (5 and 6), those concerned with occupational aspirations and with "feeling more intelligent than classmates," show no significant change differences between the METCO and "control" groups. And the non-response bias may account for the one significant change difference—in regard to the desire to obtain a bachelor's degree (Figure 4)—since the full cross-sectional samples reveal a similar decline for both groups (—11 per cent to —12 per cent).

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Two careful desegregation investigations from Pittsburgh and Evanston, however, *have* found lower black aspirations combined with *better* academic performance. Black ninth graders in Pittsburgh had significantly higher arithmetic achievement and lower educational aspirations in desegregated schools (St. John and Smith, 1969). Similarly, *both black and white pupils* in Evanston's third, fourth, and fifth grades who had previously been in predominantly black schools reported somewhat lower academic self-concept scores after two years in predominantly white schools (Weber, Cook, and Campbell, 1971; Hsia, 1971). And we have noted that Evanston's black and white children made achievement gains during desegregation, though they were not statistically significant (Hsia, 1971). Since this effect occurred for *both* racial groups, these investigators inferred that this "social comparison effect" reflected adaptation to new norms and more realistic conceptions of academic performance.

The key to understanding the apparent paradox of reduced aspirations combined with increased achievement is the well-known psychological principle that achievement motivation and aspiration level are by no means identical. Researchers have repeatedly found that moderate motivational levels are best for learning and achievement (Atkinson, 1964). Some of this motivational research directly concerns black children. Katz (1967), for example, has demonstrated experimentally how unduly high aspirations can doom black students to serious learning difficulties. In his view, desegregation benefits learning among black children by lowering their aspirations to more effective and realistic levels. Veroff and Peele (1969) supported Katz's position in a study of desegregation in a small Michigan city. They found that achievement motivation, as measured by the choice of moderately difficult tasks, significantly increased for black boys after one year in a desegregated elementary school; black girls, however, did not evince the change.

If METCO had drastically curtailed black ambitions to low levels, this would have been a negative result. But METCO reduced these ambitions only slightly, for they remained as high or higher than the ambitions of white students in METCO schools.[11] In short, when desegregation lowers rigidly high aspirations of black students to moderate, effective levels, it should be considered a positive, not a negative effect.

Shifts in racial attitudes during desegregation are exaggerated and interpreted too narrowly. "Busing" fails again, in Armor's view, because he regards his METCO data as indicating that desegregation leads to negative effects for race relations. Once again, these METCO data are tenuous at best. Though much is made of it, the increase among METCO children in their desire to attend schools with at least half-black student bodies proves not to be significantly different from a similar increase among the "control" students (Figure 7). No control data are shown for black students' relations with white students (Figure 10), even though data without control comparisons are otherwise condemned by Armor and a large segment of the "control"

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group also attended interracial schools and had contact with white students. And as already noted, the differential administration of the third attitude questionnaire in 1970 is a critical factor which probably explains at least part of the difference between the two groups.

But if these supporting data are suspect, Armor's interpretations of them are even more suspect. "Militancy" and heightened "black consciousness and solidarity" are viewed as indicating "bad" race relations, though Armor adds, "It would be a mistake, of course, to view the increased racial solidarity of black students as a *completely* negative finding" (Armor, p. 113, italics added). Similarly, support for "black power" and a preference for a school with a student body that is evenly divided between the races are believed necessarily to involve "black separatism." Even sympathy for the Black Panthers is regarded as indicative of "anti-integration sentiments"; this despite the fact that the Panthers do not support racial segregation and removed Stokely Carmichael as a member because of his insistence on racial separatism.

These interpretations involve a logical contradiction in Armor's argument. He begins his article with the famous "hearts and minds" quotation of the 1954 Supreme Court ruling against *de jure* racial segregation of the public schools; and he employs it as evidence of the powerful influence of social science upon "the integration policy model." Yet the Supreme Court was maintaining that segregation led to black self-hate. Now when he interprets his data as showing that METCO "busing" leads to racial pride, militancy, and a desire to be among blacks as well as whites, Armor concludes that "the integration policy model" is proven wrong and that "busing" causes bad race relations.

The article admits that the METCO children are still supportive of the program, but emphasizes the trend toward "militancy." No consideration is given to the effects of the differential administration of the third-wave questionnaires; nor is any given to the possible effects of the study's having begun just after the 1968 assassination of Dr. Martin Luther King, Jr., a tragic event with wide repercussions for black/white interaction. Finally, the attitude results, like the achievement results, must be reinterpreted in the light of our discovery that much of the "control" group attends substantially desegregated schools. It could be, then, that the extreme tokenism of the METCO programs influenced these attitude results. They cannot be related to "busing" and desegregation, given the composition of the "control" group.

Nonetheless, Armor views these findings as a challenge to contact theory. To buttress this contention, he selectively cites a lone finding out of context from Useem's (1971, 1972) 1969 study of white racial attitudes in METCO schools.

> Nonetheless, although the evidence is not complete, what we have indicates that the white students themselves were negatively affected by the contact. . . . [t]hose students who had direct classroom contact with bused black students showed *less* support for the busing program than

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> those without direct contact. In fact, the kind of students who were generally the most supportive—the middle-class, high-achieving students—showed the largest decline in support as a result of contact with bused black students. This finding is based on cross-sectional data and does not indicate a change over time, but it is suggestive of the possibility that a general polarization has occurred for both racial groups. (Armor, pp. 103-104)

When drawing conclusions, however, he forgets his own caution against drawing causal inferences and flatly states that "white student attitudes in the receiving schools also tended *to become* less favorable to black students . . ." (Armor, p. 112, italics added).

The simple correlation between increased classroom contact and more negative feelings toward METCO among white students is statistically significant; but Armor fails to report that the relationship is no longer significant once such variables as sex, socio-economic status, and academic standing are taken into account. Moreover, this effect is limited to upper-status students of high ability who remain favorable to the program but who have their initially unrealistic expectations of blacks modified.

There is also a failure to report other relevant findings from Useem's work. For example, she found a statistically significant positive relationship between favorable white attitudes toward METCO and earlier equal status interracial contact in elementary school, summer camp, etc.; and this strong relationship remained significant after full controls were applied. Useem also found a relationship ($p<.08$) between support for METCO and interracial contact in extracurricular activities. Moreover, she found that having a METCO friend is strongly linked to support of METCO, and is best predicted by equal status contact with blacks as a child and with METCO students in class and school activities.[12]

The evidence that school desegregation "channels" blacks into greater future opportunities is stronger than presented. The one "success" of "busing," Armor admits, is that METCO appears to "channel" its students into colleges at higher rates than control students presumably from the same families. But this finding is couched with many qualifications that are conspicuously absent from his negative conclusions. Furthermore, his article actually understates METCO's success in this regard and fails to cite recent research that indicates that it may well be an important effect of interracial education in general.

Armor's article shows in its Figure 11 that 78 per cent of the METCO graduating class of 1970 entered four-year colleges, compared to only 44 per cent of the controls. By the fall of 1971, the percentages were 66 per cent and 44 per cent; and by the spring of 1971, 56 per cent and 38 per cent. (For universities, the spring 1971 figures were even more impressive, with 43 per cent of the METCO graduates and only 12 per cent of the controls enrolled.) Similarly, positive results are cited from another special program (Perry, 1972).

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But the article also implies that the METCO drop-out rate from college is excessively high, suggesting that the program pushes into college students who do not belong there. This point is answered as soon as one compares the METCO figures with other data on college attendance. For 1969 and 1970, the percentages of the *total* graduating classes of the METCO high schools going on to four-year colleges were 61 per cent and 62 per cent—all well below the 1969 and 1970 METCO figures of 77 per cent and 78 per cent (Useem, 1971).[13] Moreover, the 84 per cent college *retention* rate of the 1970 METCO graduates who entered the second year of the four-year colleges is *not* abnormally low. In fact, it is slightly above the 78 per cent national retention rate for white students in four-year colleges (Astin, 1972).

Nor was the 1970 METCO graduating class unusual. Robert Hayden, the director of METCO, kindly supplied us with data on the 32 METCO graduates of 1969. Twenty-eight (88 per cent) entered college in the fall of 1969, while four began full-time employment. Three years later, attempts were made to contact the entire group, and 22 of the 28 college-attenders were reached. One was in the Army, and five had left college. Sixteen (73 per cent), however, were still enrolled in college.

Yet Armor belittles such concrete results. He emphasizes that such findings are tentative, based on small samples, and may indicate that the future benefits of biracial schooling are limited to the college-bound. The importance of all three of these cautions is reduced, however, by a major research effort that goes unmentioned. Robert Crain (1970), using a 1966 survey of 1,624 adult blacks in the urban North, focused upon the occupational and income outcomes of desegregated education for high school graduates.[14] Crain concludes:

> American Negroes who attend integrated public schools have better jobs and higher incomes throughout at least the next three decades of their life. The differences in income cannot be accounted for by the higher educational attainment of alumni of integrated schools, or by the higher differences in social background. The most significant effect of integrated schools is probably not "educational." It is probably more important that Negroes who attend integrated schools will have more contact with whites as adults, and tend to have more trust in whites than do Negroes from segregated schools. This in turn partially overcomes a crucial barrier to equal opportunity—the fact that information about employment opportunities is spread through types of informal social contacts to which few Negroes have access.

The firm policy conclusion against "mandatory busing" is not substantiated by the evidence presented. For the many reasons discussed above, the evidence does not justify Armor's unqualified conclusion: "The available evidence on busing, then, seems to lead to two clear policy conclusions. One is that mandatory busing for purposes of improving student achievement and interracial harmony is not effective and should not be adopted at this time" (Armor, p. 116). Inter-

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estingly, this conclusion was added to the final version after considerable publicity concerning Armor's paper had been generated by its repeated leaks to the mass media. An earlier draft had concluded only that "the data may fail to support mandatory busing as it is currently justified . . ."

Armor also concludes that "voluntary busing" should continue for those who still believe in it and for the sake of social science research. Yet he never demonstrated, nor do we detect it when reviewing the evidence, that "mandatory" and "voluntary" desegregation lead to different effects. "Mandatory busing" is condemned out of hand even though his article rests most heavily on a voluntary program's effects, and rests entirely, except for Berkeley, upon token programs with small numbers and percentages of black children, while most "mandatory" programs involve larger numbers and percentages of black children in Southern cities excluded from consideration.

In a real sense, Armor's article does not concern itself with "busing" at all, save for its title and its conclusions. It does not provide us with direct evidence on the "busing" of school children for racial desegregation, for it never treats "busing" as an independent variable. Rather, his article is an attack upon the racial desegregation of public schools that often, but not always, involves "busing." Large numbers of the children in the few studies cited by Armor attend desegregated schools without "busing." And we have noted that in his own METCO study many of his so-called "controls," who were supposed to be "unbused" and segregated, were in fact "bused" and desegregated. Furthermore, a check on his METCO sample finds that a substantial number were *not* bused. Armor was apparently aware of these problems, for he admitted in his court testimony for segregation in Detroit that "a more accurate title would be 'The Effects of Induced School Integration.'"

To our knowledge, there is actually no evidence whatsoever that "busing" for desegregation harms children. This is fortunate, since over 40 per cent of all school children in the United States are "bused" daily (though only three per cent are "bused" for puposes of achieving racial desegregation; Metropolitan Applied Research Center, 1972). Only one of the investigations mentioned in Armor's article actually utilized "busing" as an independent variable. It found, though this was also omitted, that black pupils in Evanston who were bused to desegregated schools attained significantly higher test score gains than those who either remained in or walked to desegregated schools (Hsia, 1971). This result may be an artifact of selection, but it at least indicates that "busing" *per se* did not impair achievement.

IV

The article's basic assumptions about racial change are unjustified. To this point, our critique has answered Armor's argument within the narrow confines of his view of the process of racial desegregation of the public schools. But here we wish to break out of these confines and to challenge the basic assumptions about racial change that under-

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gird his entire article. Armor's thesis is predicated on viewing school desegregation as a technical matter, an inconvenient intervention whose merit must be judged solely by how well *black* children manage to adapt to it. Blacks are once again the "object" whose reactions should determine "what is good for them." The conditions faced by black children go unmeasured and ignored, and the whole context of American race relations is conveniently forgotten. All interracial contact is assumed to constitute "integration." No mention whatsoever is made of white racism, individual and institutional, which the Kerner Commission maintained was at the root of the problem (National Advisory Commission on Civil Disorders, 1968). Nor is there any discussion of the strong argument that genuine integration is necessary primarily for its potential effects on *white* Americans and *their* racial attitudes.

Instead, the whole issue is portrayed as the creation of "liberal educators" who are "so intent on selling integration to reluctant white communities that they risk the danger of ignoring the opinion of the black community" (Armor, p. 115). Forgotten is the fact that the issue was the creation of black America, from Charles Hamilton Houston to Roy Wilkins, and that it has been continuously opposed by white America with every conceivable means.

Data from the limited METCO sample are generalized to the whole black community (Armor, p. 113). The anti-busing resolution of the National Black Political Convention held in Gary, Indiana, in March 1972 is emphasized, but the paradoxical fact that the same Convention also passed a strong "pro-busing" resolution is not cited. While it is acknowledged that "many black leaders favor school integration . . ." and that "the *majority* of blacks *may still* endorse the *concept* of integration . . ." (Armor, pp. 112, 115, italics added), the full range of support for school integration (not merely desegregation) in the black community is never revealed. "Would you like to see the children in your family go to school with white children or not?" When asked this question at the time of the METCO research in 1969, 78 per cent of a national sample of black Americans (*up* from 70 per cent three years before) chose "go with whites," as opposed to 9 per cent "not with whites" and 14 per cent "unsure" (Goldman, 1970).[15] Thus not just a majority but an overwhelming portion of black America still opts for school integration. If any further evidence were needed, the immediate and hostile public reactions of many blacks to the initial newspaper stories concerning Armor's paper should have supplied it. This is not to deny that there are strong doubts among blacks, especially the young, as to whether white America will ever allow genuine integration to become the national norm, doubts that are only reinforced by the assumptions upon which Armor's article is based.

Armor asserts that the burden must fall upon those who support school integration to prove that it works. Given America's unhappy racial history, we believe that the burden of proof rests with those who wish to maintain racial segregation. But actually such contentions miss the point. The courts' interpretation of the 14th Amend-

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ment of the United States Constitution, and not social scientists' opinions about black responses, ultimately governs the racial desegregation of the public schools and court-ordered transportation which may be needed to achieve it. This fundamental fact was dramatically demonstrated by the judicial reaction to Armor's deposition in the Detroit school case, a deposition based on an earlier draft of "The Evidence on Busing." On June 12, 1972, U.S. District Court Judge Stephen H. Roth ruled the deposition inadmissible as evidence on the grounds of irrelevancy. The deposition, in Judge Roth's view, represented "a new rationale for a return to the discredited 'separate but equal' policy . . ."

FOOTNOTES

[1] This is true from the early statements on the desegregation process by Clark (1953), Williams and Ryan (1954), Johnson (1954), and others (summarized in Coleman, 1960) to more recent statements by Katz (1964) and Pettigrew (1969, 1971).

[2] Matthai (1968) describes the White Plains (1967) research as follows: "The small numbers of Negro students tested (33 desegregated students, 36 from previous years); the lack of explicitness about comparability of the groups under study and the rationale of sample selection; the occasionally contradictory figures and tables; the lack of significance tests; the selection of only one grade level for study (plus a truncated comparison of another grade level); and the almost impenetrable prose of the research report make this study utterly equivocal."

[3] Grades two and four were excluded because of problems of sample drop-out. Earlier work showed somewhat greater gains for the desegregated youngsters in the second grade and for the segregated youngsters in the fourth grade (Mahan, 1968), so the omission of these two grades should not bias the results of this new analysis (Thomas Mahan, personal communication).

[4] More recently, a study has been released by the Center for Urban Education concerning 25 black first, second, and third graders bused under Project Concern from Bridgeport to Westport, Connecticut. Though the sample size renders its findings tentative, it found marked academic improvement for the "bused" children during one-and-a-half years when compared with similar unbused children remaining in the segregated sending school in Bridgeport. The study also found no ill effects among the desegregated white children (Heller *et al.*, 1972).

[5] We wish to thank Robert Hayden of METCO, the Boston School System, and the families of the children contacted for their helpful cooperation in securing these data.

[6] We are here following the standard practice of defining a segregated school as one with a predominantly black student body. Had we employed a majority-white definition for a desegregated school, the "control" percentage attending desegregated would be 53 per cent (29/55) instead of 62 per cent (34/55). Small numbers of Chinese-American and Spanish-speaking students in a few of the schools explain the minor difference.

[7] Our projected sample sizes conservatively assume a standard deviation of the junior high gain scores of one grade level.

[8] Unfortunately for the discerning reader, Armor failed to mention these losses of elementary subjects in the one footnote he devotes to the subject. We obtained them from Walberg (1969).

[9] Using the Coleman report data, the standard deviation for groups of white students in desegregated schools in the Metropolitan North is only about 40 per cent as large as the standard deviation of the white individual scores; or, on Coleman's verbal test, roughly four points where the standard deviation of the individual whites is 10 points (Coleman *et al.*, 1966). Since Armor finds that the mean for white groups in desegregated schools is roughly one-and-a-half group mean standard deviations larger than that for black groups in desegregated schools, we estimate that the average black child is roughly six points (1.5 x 4

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points) behind the average white child. Translating this into individual percentiles and assuming that the average white in desegregated schools is at the 50th percentile, we arrive at our estimate that the average black pupil in desegregated schools is between the 25th and 30th percentiles.

[10] Bailey (1970) has also shown that high school "disruptions" and racial tensions are far less likely to occur when the black staff percentage is equal to or greater than the black student percentage.

[11] Useem (1971) studied white tenth graders' aspirations and attitudes in eight out of the nine secondary schools participating in the METCO program during 1968-69. She found white aspirations just equal to or below those reported for blacks in the same schools. Thus, 74 per cent of the white students wanted to be above the middle of the class academically compared to about 80 per cent of the black students; and 26 per cent of the whites aspired to a professional or graduate school compared to 35 per cent of the blacks.

[12] In his Detroit segregation testimony, Armor stated that he omitted these positive findings of contact because they were voluntary and therefore could have been caused by self-selection. But classrooms at the high school level often involve selection too. Besides, 72 per cent of Useem's white students who had contact with METCO students in school activities had it in athletics. Armor's argument requires us to believe that tolerant white students would go out for football primarily to have contact with the few black players on the team.

[13] Data from one METCO high school was unobtainable for 1970, but the similarity of the percentages for the two years suggests that this does not introduce a serious bias.

[14] From these same data, Crain (1971) also finds "that those who attended integrated schools are more likely to have graduated from high school, are more likely to have attended college, and score higher on a verbal test than those who attended northern segregated schools. It seems likely that the higher achievement of Negroes in integrated schools can be attributed partly to differences in the character of their classmates, irrespective of race. In addition, however, there is evidence that attending integrated schools has an important impact in establishing social and psychological preconditions for achievement."

[15] Armor's data on black attitudes toward "busing" in his footnote 11 are outdated. By March 1972, blacks favored "busing" for integration by 54 per cent to 34 per cent (Harris, 1972).

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The Double Double Standard: a Reply

DAVID J. ARMOR

THOMAS PETTIGREW and his associates have missed the essential point of my study. As a consequence, their comments shed little light on the current public controversy over busing. Indeed, their critique further promulgates the ambiguities and confusions that have prevailed in the field of race relations since Myrdal's *An American Dilemma.*

The essential requirement for sound reasoning in this matter is observance of the distinction among the findings of science, the results of policy, and the dictates of law or morality. I studied the results of existing policies of induced school integration (all of which used, of necessity, varying amounts of busing). I was *not* studying the scientific issue of what *might* happen under various conditions (other than those in effect in the programs studied), nor the legal question of whether it *should* have happened according to various constitutional interpretations. My task was far simpler. I asked only the question: What *has* happened? My critics have confused the *has* with the *might* and the *should.* This confusion is further compounded by their application of two double standards for the evaluation and use of the evidence on busing.

I am accused of having too severe standards and unrealistic expectations about the benefits of induced school integration (which I will hereafter abbreviate as "busing"). But *I* did not formulate these standards and expectations. They come from the programs themselves, buttressed by several noteworthy studies, particularly the Coleman report and the 1967 report of the U.S. Commission on Civil Rights. I do not doubt that existing busing programs are also based upon moral and legal principles, especially the 1954 Supreme Court doctrine that "separate is unequal." But even in the 1954 decision social science findings are cited as "authority" and hence become entangled with constitutional issues.

One expectation stands out above all others: Integrated education will enhance the academic achievement of minority groups, and thereby close (or at least substantially reduce) the achievement gap. There is good reason for the prominence of this belief. The Coleman study revealed a large and consistent achievement gap between white students and most minority groups (with the notable exception of Oriental students). The gap between black and white students av-

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erages about 33 percentile points. This means that for any black child and white child drawn at random from the general population, we can expect the black child's scores to average about 33 percentile points below the white child's. This achievement gap became the main argument against segregated education and the yardstick by which to measure progress. It is unlikely that de facto segregated education would ever have become such a major issue, or that so many communities would have voluntarily initiated busing programs, without this evidence.

THIS is also the central issue in the critique. The critique makes the incredible claim that looking at black and white achievement differentials is not appropriate, since *both* groups may gain under integration. Not only is there little evidence in support of this claim, but even if it were true there is no way we could conclude from it that integration would solve the educational deprivation of minorities. Would we solve the economic problems of minorities if we raised *everyone's* annual salary by $3,500? Of course not. Such a gain was in fact registered by both whites and non-whites between the 1960 and the 1970 census, but there has been no lessening of the clamor over economic inequality. But money at least has some meaning in absolute terms; this is not the case for academic achievement as measured by testing. As any educational specialist knows, there is no "zero-point" on an achievement test, and progress is always measured on a *relative* basis (i.e., a student's progress relative to a national or local norm). Thus if the black/white achievement gap does not change, there is no way one could conclude that busing is beneficial for minority groups.

I am accused of selecting only "negative" studies and leaving out seven other adequately-designed studies that were more "positive." In fact, I looked at all the studies that I could obtain at the time. Their results were so consistent that I was quite confident about my conclusions. I have now looked at these seven reports (only four of which meet the technical requirements for an adequate study) and have no reason to change my conclusions; nor do I see much evidence to support the authors' optimism.

The only way to settle this issue is to look at some of the findings. I have selected a number of studies that were not in my original review, including some that are cited by Pettigrew and his colleagues. The only criteria I used for my choices were the comprehensiveness of the data and the presence of some of the conditions my critics claim are important for achievement gains (i.e., two-way busing, classroom integration, duration, etc.). I will focus on reading achievement, since this is about the only academic skill which is measured in all of the studies.

The first example is drawn from the Evanston study, which in my opinion is technically one of the best. Also, it fulfills most of the important conditions cited in the critique: A sizable proportion of the students were black (about 20 per cent); almost all classrooms were racially balanced; faculties were integrated (about 10 per cent

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black); and the duration of the integration experience was three years. The performance of the fourth-grade cohort is typical:

TABLE 1. *Reading Achievement in Evanston*[1]

RACE	BEFORE INTEGRATION (GRADE 4—1967)	AFTER INTEGRATION (GRADE 7—1970)
White (N=185)	253	278
Black (N=606)	237	253
Gap	**16**	**25**

1 Adapted from Jayjia Hsia, *Integration in Evanston* (Evanston: Educational Testing Service, 1971), Table 11. Scores are based on the STEP reading test; the standard deviation is approximately 15.

The black/white gap is 16 points before integration, or just about one standard deviation (almost identical to Coleman's finding for the sixth grade nationally). After three years of integration the gap has increased to 25 points, and we can see that the *black students in grade seven are performing at the same level that the white students were at in the fourth grade.* In other words, in the seventh grade the black children are three years behind white children in reading achievement. Similar results were found for cohorts starting at grades one and five and for performance on arithmetic achievement tests. We do not know whether the achievement of *both* groups might have been enhanced; but what difference would that make in terms of the possible harmful effects on the black children in Evanston who are forced to compete for academic rewards at so large a disadvantage?

The Berkeley data also afford a good example, for the Berkeley program employed two-way busing (whites to previously majority-black schools and vice versa) and integrated faculties and classrooms. Although the study was cross-sectional, data were presented over a four-year period for six grade levels; thus it is possible to construct a first-grade cohort and follow that same grade (if not exactly the same students) through two years of integration experience:

TABLE 2. *Reading Achievement in Berkeley*[1]

RACE	BEFORE INTEGRATION (GRADE 1—1967)	ONE YEAR OF INTEGRATION (GRADE 2—1968)	TWO YEARS OF INTEGRATION (GRADE 3—1969)
White (N=500+)	1.9	3.1	4.1
Black (N=400+)	1.6	2.2	2.8
Gap	**.3**	**.9**	**1.3**

1 Adapted from Arthur D. Dambacker, "Comparison of Achievement Test Scores Made by Berkeley Elementary Students Pre and Post Integration" (unpublished report, Berkeley Unified School District, 1971), Table 7. Scores are grade equivalents based on the same test—the Stanford Achievement Test (administered in May each year).

We can make inferences about these data if the student turnover rate is not too high, which is a reasonable assumption. In each year the

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gap increases, so that after two years of integration the gap is more than one grade level. Again, it is clear that integration has not closed the achievement gap in Berkeley, and the black students are competing at a large disadvantage.

Sacramento is one of the integration programs cited by the authors as indicating positive effects of integration. While it is true that there are some positive results reported for *some* tests, the black/white gap does not change. The following data are for the first-grade cohort:

TABLE 3. *Reading Achievement in Sacramento*[1]

GROUP	BEFORE INTEGRATION (MAY 1966)	AFTER INTEGRATION (MAY 1967)	AFTER INTEGRATION (MAY 1968)
Majority (N=221)	2.1	3.2	4.1
Minority (N=35)	1.6	2.0	2.9
Gap	**.5**	**1.2**	**1.2**

1 Adapted from Albert J. Sessarego, "A Summary of the Assessments of the District's Integration Programs, 1964-71" (unpublished report, Sacramento City Unified School District, 1971). Scores are grade equivalents based upon the Stanford Reading Test. Minority group includes both black and Mexican-American students.

The resemblances to the Berkeley data are striking. Again we see that while the gap has not widened, it exceeds a whole grade level by the end of the third grade. Sacramento has also reported some interesting data which allow comparison of segregated minority students receiving intensive compensatory services with integrated minority students. Averaging over the Stanford Reading Test in grades one to six, we find that the compensated segregated students gained about 1.1 years, while the integrated students gained about 1.0 years. In other words, it is possible that the slight improvements Sacramento observed in achievement of integrated students compared to *non-compensated* segregated students (for some grades on some tests) are due to differences in the services of instruction received at the integrated schools and not to integration per se. While Coleman found that school facilities and staff were not major contributors to achievement differentials, he did not say that they had *no* effect whatsoever.

Another "positive" example cited by the critique is a study of integration via school "pairing" in New York City in 1965. This study is particularly interesting in that an attempt is made to compare integrated students with both black and white segregated students. While the study gives no indication about classroom or faculty integration (which are important for educational benefits, according to my critics), and while the paired school is not majority-white (another supposedly crucial condition), it does afford us a look at the black/white gap in reading achievement for a fifth-grade cohort. As can clearly be seen in Table 4, for the integrated students the achievement gap is large (starting at almost three grade levels) and increases (to almost three and one-half grade levels) after one year of integration. The "positive" result in this study is that the integrated

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TABLE 4. *Reading Achievement in New York*[1]

SCHOOL	RACE	BEFORE INTEGRATION (APRIL 1965)	AFTER INTEGRATION (MAY 1966)	GAIN
Integrated, paired school	White (N=30)	6.8	8.5	1.7
	Black (N=32)	4.0	5.1	1.1
	Gap	**2.8**	**3.4**	
Segregated schools	White (N=57)	5.7	7.2	1.5
	Black (N=80)	3.5	4.4	.9
	Gap	**2.2**	**2.8**	

1 Adapted from I. W. Stone, *The Effects of One School Pairing on Pupil Achievement . . .* (unpublished Ph.D. Thesis, New York University, 1968), Tables 18 and 20. Scores are grade equivalents on the Metropolitan Achievement Reading Test.

black students gained 1.1 grades (or 11 months) while the segregated black students gained .9 (or nine months). It does not seem to me that this difference provides much ground for optimism, particularly since the segregated white students also gained about two months less than the integrated white students. That is, the slight difference we observe might be due to differences in instruction content or style and not due to the effect of integration.

The argument of Pettigrew and his colleagues that perhaps white students also gain in achievement from the integration experience per se demands close scrutiny. While it makes sense to argue that black students might gain by being in a classroom environment with higher-achieving white students (the so-called "peer" effect prominent in the Coleman study), it makes no sense at all to argue that white students will gain by being in a classroom environment with lower-achieving black students. What mechanism could possibly be operating that produces *opposite* peer effects for the two groups? It seems to me that my critics' reasoning is getting fuzzy here.

But this is not the crucial issue anyway. One of the main points of my study was to show that black achievement is not being helped in any significant way by busing, and that therefore we have to raise the possibility of harmful psychological effects due to the achievement gap. The small gain of two months for the paired black students in New York is little consolation for their being placed in an environment where they must compete for grades with students *three years ahead* of them in academic growth. The authors completely ignore this issue throughout their critique.

The critique cites another, more recent study of Project Concern (Hartford and New Haven) that shows more positive results. I originally described the Project Concern studies as showing "mixed" results. The new study does not change my view; in fact, it bears great similarity to the other studies presented here. Like the New York study, it presents results for both races in both integrated and segregated environments. It is a particularly good example in that the bused pupils received a variety of compensatory services (such

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as minority teachers and aids recruited from the sending school). The results for the second grade are typical:

TABLE 5. *Reading Achievement in Project Concern*[1]

SCHOOL	RACE	BEFORE INTEGRATION (1971)	AFTER INTEGRATION (1972)	GAIN
Integrated school	White (N=22)	3.4	4.7	1.3
	Black (N=9)	2.1	2.9	.8
	Gap	**1.3**	**1.8**	
Segregated schools	White (N=20)	3.9	5.2	1.3
	Black (N=16)	1.8*	2.4*	.6*
	Gap	**2.1**	**2.8**	

1 Adapted from Barbara R. Heller, *et al.*, "Project Concern, Westport, Connecticut" (New York: Center for Urban Education, 1972), Table 3. Scores are grade equivalents on the Metropolitan Achievement Reading Test. The asterisks indicate that the scores for the segregated black students have been adjusted to reflect a shorter testing period.

Again, we can see that the achievement gap increases for the integrated students, starting out at 13 months and ending at almost two years. We have very much the same situation as in the New York pairing study; the integrated black students gain slightly more than the segregated black students (two months), but the achievement differential is still large and increases over the year. Notice, however, that in this case the segregated white students gain as much as the integrated white students.

My critics cite other studies not presented here. As I have already said, three of them (Rochester, Goldsboro, N. C., and Newark) did not qualify according to my criteria for an adequate study; they did not use the same achievement tests both before and after integration. The Philadelphia study is of limited utility since it dealt only with black students with very high I.Q.'s. The Buffalo study showed mixed results, with one grade showing greater gains for integrated, one grade showing greater gains for segregated, and a third grade showing a small (two months) gain for integrated black students. But in all three grades the white integrated students showed even greater gains, indicating the same increasing achievement gap seen in the other studies.

In view of all of these studies, I can see no reason to change my conclusion that "to date there is no published report of *any* strictly educational reform which has been proven substantially to affect academic achievement; school integration programs are no exception." It was my purpose to show that existing programs have not demonstrated a consistent and important effect on various expected benefits (especially achievement). It was not my intention to *prove* that achievement *could not* be affected, only to show that it *has not* been affected by *existing* programs. Therefore, my critics' argument that the programs I looked at did not fulfill the proper conditions for integration is beside the point. But I will go further than that:

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They have presented no convincing evidence that any programs—even those fulfilling their conditions—are having an important effect. There is no clear evidence in the studies mentioned that they fulfilled their conditions, nor is there *any* evidence in these studies—regardless of the conditions—that school integration will close the achievement gap by "approximately a fourth." Of course, it is still true that, under some conditions, integration *might* have an effect. But those who believe this premise will have to produce far better evidence than is currently available.

THE methodological critique of the Boston METCO study is equally irrelevant to my conclusions and recommendations. I would never have made policy statements based on the METCO research without seeing a considerable amount of supporting evidence. I think the reader can see from what has been presented that there is, indeed, a great deal of corroboration. Methodological critiques are always liable to a common fallacy: The existence of technical weaknesses in a study does not prove the converse of its findings. I believe in the METCO findings because they were consistent with many other studies, not because the METCO research was infallible. I am certainly cognizant of some of the limitations of the METCO research pointed out by the critique. Any single social science study could be given a similar treatment. Research conditions in policy evaluation studies are seldom ideal; this is why a social scientist must look for consistency across many studies before coming to any conclusions.

I do not agree with all of the criticisms of the METCO study made by Pettigrew and his associates. In particular, I take issue with their statement that many of the METCO control group students attended integrated schools and therefore were not a proper comparison group. Our control groups were screened for attendance at Boston public schools in the black community, most of which are predominantly black (particularly the elementary schools). Moreover, even those few control group students whose neighborhood school is majority-white still provide a proper comparison, not only because the proportions of minority and lower-class white students are higher in these schools, but also because the Boston schools are presumed unable to provide the kind of quality education found in middle-class suburban schools. After all, this is the whole rationale behind METCO and similar programs, and it must be the belief of many black parents who participate in METCO even though their children could go to majority-white neighborhood schools in Boston.

But the data presented in Table 2 of the critique are misleading in other respects. First, the authors did not use the complete METCO research records to identify schools attended by the control sample; instead, they tried to track down students using incomplete listings of students in a Boston public school register. Not surprisingly, then, they have no data on many of the control students—particularly those in the critical elementary grades. Second, it is not stressed that many of the secondary school control students were in transitional neighborhood schools with large and growing proportions of minority stu-

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dents. For example, of the 10 senior high students listed as integrated, four attended a "border-area" high school with an increasing minority enrollment of 27 per cent in 1968 and 34 per cent in 1969; another attended a high school whose minority enrollment increased from 42 to 64 per cent during these two years; and two others attended a school with a 43 per cent minority enrollment.

I undertook a complete examination of the original research records using questionnaires filled out by METCO parents in 1970 just prior to the second year of the research. Of the 36 (out of 41) elementary control students for whom there were reliable data, only 13 can be identified as attending predominantly white schools. Of the 23 students attending majority-black schools, only five attended schools with a substantial proportion of white students (all of whom were in one school whose minority enrollment increased from 53 per cent in 1968 to 64 per cent in 1969). In other words, in the elementary grades—which are, according to the critique, the more crucial years for achievement changes—complete records indicate that nearly two thirds of the control students attended segregated schools.

What is especially misleading (if not irresponsible) about all this is the authors' use of their incomplete data to conclude that it "renders [my] METCO research of no scientific interest in the study of busing and school desegregation." The clear implication here is that the control group students who went to predominantly white schools might have made large achievement gains which overshadowed lesser gains made by control group students in segregated schools. This would, in turn, make the control group gains spuriously high, perhaps even to the point of masking gains made by METCO students. But we do not have to engage in a lot of verbiage and speculation about this; we can examine the relevant elementary data directly:

TABLE 6. *Reading Achievement Gains for METCO and Control Students in the Elementary Grades*[1]

GROUP	GRADES 3 AND 4	GRADES 5 AND 6
Control students in segregated schools (N's=8 and 10)	.2	.8
Full control sample in original study (N's=14 and 27)	.3	.7
METCO as reported in original study (N's=88 and 59)	.4	.5

1 All figures are achievement gains in grade equivalents. For the fifth and sixth grade group, the five students attending the 53 per cent minority school have been excluded for the sake of purity; if they are included, the average gain for the controls in segregated schools actually drops to .7 years.

The data show clearly that the segregated control students do not differ in any important and consistent way from the full control sample (or the METCO sample, for that matter). A similar result also occurred for the junior high students; the high school student sample was too small to make any certain conclusion. What this means, then,

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is that *my original conclusion—that METCO achievement gains are not consistently larger than the control group—also holds when the control group consists only of those students attending inner-city segregated schools.* As has been so often the case throughout this discussion, when rhetoric is replaced by hard, objective data, there does not appear to be very much of substance in my critics' arguments.

THE other major finding with which the critique finds fault is that race relations seem to worsen as a result of induced school integration. Pettigrew and his colleagues seem to be somewhat ambivalent on this point. On the one hand, they criticize my conclusion on methodological grounds, such as the fact that the third-wave questionnaire was given in the white school for the METCO students and at home for the control students. (They ignore the fact that the second wave—which was given under the same conditions as the first wave—already revealed the trend of increased separatism among METCO students.) This would make one think they believe that contact does not increase racial prejudice and hostility. But at the same time they argue that the various indicators I used actually reflect "positive" changes in black self-respect—and therefore do not run counter to the expectations fostered by the integration policy model. Let me take up these two different perspectives in order.

My conclusions on race relations, like those on achievement, were not based only on METCO data. There was support from both the Useem and Riverside studies; but more important, an entirely distinct study of school integration, using the identical separatism index that was employed in the METCO research, gave strong supporting evidence. This study was a cross-sectional evaluation of "A Better Chance" (ABC), a program that places talented black high school students in white prep schools. Its data were not ready in time for use in my original article, but I can report the relevant figures now:

TABLE 7. *Black Separatism in the ABC Program*[1]

GRADE	BLACK ABC PREP SCHOOL STUDENTS	BLACK SEGREGATED SCHOOL STUDENTS	WHITE PREP SCHOOL STUDENTS
Tenth graders (N=135, 130, 134)	1.4	1.3	1.1
Twelfth graders (N=125, 137, 103)	1.7	1.0	1.0

1 Adapted from George Perry. Scores are from a separatism index ranging from 0 to 4, where 4 means most separatist. The differences between the black ABC and public school students is not significant in the tenth grade, but is significant at beyond the .001 level for the twelfth graders. The vast majority of black ABC students joined the program in the tenth grade.

We see that the twelfth-grade ABC students (most of whom started in the tenth grade) score 1.7 on the index while their tenth-grade counterparts score 1.4. The black control groups (almost all of whom attend predominantly black schools) actually show the *opposite* trend from 1.3 at the tenth grade to 1.0 at the twelfth grade. The data

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are cross-sectional (that is, the twelfth graders are not the same group as the tenth graders), so we cannot claim a causal confirmation from this study alone. Nonetheless, the ABC tenth and twelfth graders are very similar in most important respects, and the public school control sample consists of black students matched with the ABC students on important characteristics such as ability and family background. Therefore, given the identical findings in the METCO research, I must conclude that there is a strong likelihood that induced school integration enhances separatist ideology as measured by my index.

But is this convergence invalidated by technical weaknesses in the METCO study? The critique is correct in pointing out that the attitude questionnaires were given to the METCO and control students under different conditions in the second year of the study. It also calls attention to the fact that a substantial portion of the control students at the junior and senior high levels (the only levels to take the attitude tests) attended majority-white schools. But the critique fails to note that this "weakness" of the original study can actually be used to further test contact theory by comparing integrated control group students with segregated control group students—both groups having filled out questionnaires under identical conditions:

TABLE 8. *Black Separatism Gains for METCO and Control Students in the Secondary Grades*[1]

	CONTROL STUDENTS IN MAJORITY-BLACK SCHOOLS	CONTROL STUDENTS IN MAJORITY-WHITE SCHOOLS	METCO STUDENTS
Gain	—.1	.3	.4
N	(17)	(16)	(135)

1 Gain scores for the separatism index reported in the original study for a two-year period. The negative change means that separatist attitudes declined.

In my original study, I reported an over-all gain for the control group of .1. It can now be seen that the slight increase in separatism for the control group was actually due to the subgroup of students in inner-city integrated schools; their gain of .3 is almost as large as the .4 gain recorded for the METCO students. The segregated black students actually *declined* in their separatism scores—much as would be predicted by the ABC data presented earlier. Whatever interpretation one wishes to apply to these results, it seems clear that the METCO finding reported in the original study is not simply an artifact of questionnaire administration or of a faulty control group.

The available evidence supports the conclusion that induced school integration, by enhancing black identity and solidarity, may increase separatism and racial hostility; no evidence is presented by the critique that shows the converse. But is this a negative finding? I admitted in my original study that it might not be interpreted as such; on this point I obviously have no quarrel with my critics. I do,

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however, maintain that *this is not an expected finding*, either according to social science (which has long held to the Allport thesis that contact will reduce prejudice) or according to educational policy makers, most of whom stress the beneficial contribution of contact to racial understanding and harmony. And if it is contrary to expectations, it seems to me that this has very definite policy implications. Although the Supreme Court intended its 1954 ruling in favor of school integration to improve the self-concept of black people, it is highly doubtful that it expected this to be done at the expense of an increase in black hostility toward whites or white hostility toward blacks.

It seems clear that the biggest difference between my perspective and that of the critique is in regard to the policy implications of all this research. They have failed to show that the findings in my original five-city study were untrue; they have not provided convincing evidence that other programs have succeeded where these have failed; they have ignored the possibility of harmful effects. In short, their opposition to my recommendation against mandatory busing is based mainly upon the *possibility* that under certain conditions induced school integration *might* have substantial beneficial effects on minority students. In this regard, given Pettigrew's well-known use of social science findings in support of integration, their conclusions rely heavily upon the application of a double double standard.

Their belief in the possibility of educational benefits rests upon their highly questionable rejection of black and white achievement comparisons and upon a variety of small and inconsistent fluctuations in the achievement of bused students. This leads them to hold that my "firm policy conclusion against 'mandatory busing' is not substantiated by the evidence presented." Apparently, then, their view is that mandatory busing (or induced integration), whether ordered by the courts or by a local school board, is strictly a moral and constitutional issue and does not require any justification involving educational benefits. They have therefore placed the burden of proof not upon those who back the social intervention but upon those who object to the intervention.

I cannot agree with the assumptions behind this reasoning, with the kind of morality it represents, or with the implicit suggestion that social science should be used only when it favors the values of the social scientist. There is no doubt in my mind that our democratic values prohibit laws or actions that force the separation of racial or ethnic groups; I believe that the 1954 decision of the Supreme Court aimed to eliminate this compulsory separation of the races in the schools. But I also believe that compulsory integration—in the absence of clear evidence that the segregation was itself purposive and mandatory—gains little support from these same democratic principles. This is why most legal decisions and policy actions in the school desegregation movement have rested very heavily upon the assumed educational benefits of integration. In the absence of a clear constitu-

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tional or moral mandate to force racial balance in regions of de facto segregation, supporters of school integration turned to social science —where there was an unending (and unquestioned) supply of documentation of both the damage from racial segregation and the benefits of integration. This was the case in the 1954 decision (even though forced segregation was at issue); I believe it was true for the 1967 report of the U.S. Commission on Civil Rights, as exemplified in its summary statement that the "conclusions drawn by the U.S. Supreme Court about the impact upon children of segregation compelled by law . . . applies to segregation not compelled by law"; and I believe it is true for the present critique, which tries very hard—but without success—to challenge the findings of current research on induced integration.

But it follows that if the current research does not support the thesis of educational benefits, the policy must be questioned. Since the intervention has been based upon what I would call "preliminary" social science findings (very little of the data until recently was based on studies of *actual* induced integration), the burden of proof must fall upon those policy makers who support mandatory busing. The first double standard of the critique, then, is the burden of proof: To initiate the action one can use any type of social science data, whether or not it directly tests the policy in question and regardless of its technical adequacy. But once the integration policy is in full force, it cannot be questioned unless one can *conclusively prove* that school integration *cannot* have an effect on educational benefits. As far as I am concerned, the current data are far more adequate to test the efficacy of integration than was the research that existed prior to induced integration programs. Since it can in no way be concluded that the original research *proved* the existence of educational benefits, my critics clearly apply a double standard when they claim that the absence of benefits has not been proven and therefore we should not decide against mandatory busing.

THE second double standard is applied by the critique's assertion that the whole matter is really a constitutional issue, to be decided by "the Court's interpretation of the 14th Amendment." The double standard here is obvious. One willingly applies social science findings to public policy if they are in accordance with one's values, but declares them irrelevant if they contradict one's values. Pettigrew's resort to this tactic recalls a press conference reported in the *New York Times* on June 11, 1972, in which Dr. Kenneth Clark—whose scientific research and assistance was so important in the 1954 Supreme Court decision—was quoted as saying that "courts and political bodies should decide questions of school spending and integration, not on the basis of uncertain research findings, but on the basis of the constitutional and equity rights of human beings." The double standard could not be expressed more graphically.

It will be disastrous for the social sciences if they allow themselves to be used in this way. We social scientists depend upon society for our existence; our credibility is undermined if we do not present and

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use our findings in a consistent manner. The responsible use of social science in policy matters requires that we state the facts as they occur, no matter how painful their implications. And if we are willing to use facts to initiate policy reform, we must likewise use them to question existing policy. I believe that in the long run society will benefit more from decisions based on facts than from ideology contradicted by facts.

I do not want to imply that we should engage in social intervention only when it is supported by social science or stop any social intervention when the findings of science question its support. Social science cannot be brought to bear on all issues of policy, sometimes for technical reasons and sometimes for ethical reasons. Some policies cannot be researched, and some policies are demanded by constitutional principles or by common morality. But when policies are based upon empirical considerations that social science can study, there is a way that policy and science can proceed in concert. That way utilizes the method of social experimentation and evaluation—a method that has long been prominent in the medical sciences. We would not think of prescribing a new drug without first obtaining sound evidence of both its efficiency and its harmlessness by experimental evaluation of its actual effects on human subjects (usually volunteers). Why should not a similar standard be applied to proposed remedies for curing social ills? Our assumptions about social behavior have been proven wrong in the past, and they will be proven wrong in the future. The only way to make reasonably sure that the remedy is not worse than the malady is to engage in careful research under realistic conditions. That our government is beginning to adopt the principle of social experimentation is shown by Congress's recent decision to perform a large-scale, long-term experiment to test the efficiency of a guaranteed income plan before implementing it for the whole nation. This is a welcome sign for those who want to see a closer connection between social science and public policy.

It is this kind of philosophy that led me to favor voluntary busing programs, not any evidence that voluntary busing is more efficacious than mandatory busing. I do not think the evidence pointing to an absence of educational benefits or the evidence for possible harmful effects is strong enough to justify a prohibition of busing for those families and communities that desire it—regardless of their motives. On the contrary, I would like to see more voluntary busing on a controlled, experimental basis accompanied by a careful research and evaluation effort. This is the only responsible way to resolve the controversy and to establish sound guidelines for policy makers.

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On Pettigrew and Armor: an Afterword

JAMES Q. WILSON

THOSE who have read David Armor's "The Evidence on Busing" and now find in this issue a lengthy rebuttal by Thomas Pettigrew and colleagues and a surrebuttal by Armor might be forgiven for throwing up their hands in despair at the apparent inability of social science to give clear and simple answers to important questions. One is powerfully tempted to decide that social science has nothing to say —or worse, too much that is inconclusive to say—about matters of public policy. Why not, one might ask, let the question of desegregation and busing be decided entirely on the basis of what one feels is right without regard to scholarly haggling?

Though understandable, *The Public Interest* feels that such an attitude is mistaken—as mistaken as the attitude that scientific findings can be decisive on matters of social choice. Social science, if carefully done, can under certain conditions measure the relationships among two or more elements of a social situation, and more often than its critics suppose discover relationships that are contrary to popular supposition. When the late Samuel Stouffer taught his sociology course at Harvard, he often began by giving the students a quiz in which they were asked to decide whether certain statements were true or false—and of course the students tended to pick the "obvious" answers, all of which were contrary to scientifically-established fact.

The circumstances under which social science can produce non-obvious, non-trivial, valid findings are unfortunately not as commonplace as we would like. One must be able to define and measure with some precision the factors under consideration, to study or manipulate them without changing them in unknown ways, and to isolate them either experimentally or statistically from the influence of other factors. When these circumstances occur and where there exists financial support for such inquiries, social science prospers—as it has in voting studies and market research. The effects to be explained (a vote, a consumer purchase) are unambiguous and easily measured, the factors generally thought to produce these effects (the income, education, religion, or occupation of the voter or consumer) are also easily measured, the carrying out of the study (at least after the fact) does not alter the decision, and political candidates and business firms are willing to pay for the results.

Studies of the effects of segregated or desegregated schools (or even studies of schools, period) rarely meet all of these criteria; neither do efforts to understand the causes of crime, of persistent unemployment, of broken families, of drug addiction, and of racial

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differences (if any) in intelligence. Either the effects to be studied are hard to measure (as with educational attainment or true crime rates), or the possible causes are hard to define and detect (as with most habits of mind and of personality), or the possible explanatory factors are hard to disentangle (as with race, class, and education), or the act of studying the situation alters it (as when persons who are part of an experiment come to like—or dislike—the special attention that is being paid to them).

Because of these considerations, and after having looked at the results of countless social science evaluations of public policy programs, I have formulated two general laws which cover all cases with which I am familiar:

> **First Law:** All policy interventions in social problems produce the intended effect—*if* the research is carried out by those implementing the policy or their friends.
>
> **Second Law:** No policy intervention in social problems produces the intended effect—*if* the research is carried out by independent third parties, especially those skeptical of the policy.

These laws may strike the reader as a bit cynical, but they are not meant to be. Rarely does anyone deliberately fudge the results of a study to conform to pre-existing opinions. What is frequently done is to apply very different standards of evidence and method. Studies that conform to the First Law will accept an agency's own data about what it is doing and with what effect; adopt a time frame (long or short) that maximizes the probability of observing the desired effect; and minimize the search for other variables that might account for the effect observed. Studies that conform to the Second Law will gather data independently of the agency; adopt a short time frame that either minimizes the chance for the desired effect to appear or, if it does appear, permits one to argue that the results are "temporary" and probably due to the operation of the "Hawthorne Effect" (i.e., the reaction of the subjects to the fact that they are part of an experiment); and maximize the search for other variables that might explain the effects observed.

People will naturally disagree over whether a given policy evaluation by a social scientist supports either the First Law or the Second Law. Many considerations prevent that argument from being carried on very intelligently—the loyalties and commitments of the scholars involved, the efforts of partisans and polemicists to defend one interpretation absolutely and to reject the other entirely, the defensiveness of whatever government agency is being praised or blamed by the study in question, and the tendency of human affairs to be so complex and ambiguous as to make the possibility of designing and executing a Decisive Experiment all but impossible.

If this is the case, on what grounds can anyone defend such policy-evaluating social science as exists? In part, because some studies *do* provide answers, even when judged by the most rigorous standards

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—it is a sad but clear fact, for example, that the reading scores of black children in big-city elementary schools lag significantly behind white scores. (What is at issue is not the difference, but what causes it and what may be done about it.) But in large part social science evaluations, and the debates over them, are useful because they expose the complexities of a problematic situation, extend the range of possible explanations for those conditions, increase our awareness of the unintended as well as intended outcomes of any policy intervention, and stimulate us to reflect on the inadequacies of our own preconceptions about the matter. In short, serious social science, seriously debated, can be a civilizing influence, despite the fact that some of its critics regard the very effort to be scientific as uncivilized.

Social science begins by attempting to simplify human affairs in order that they can be more easily explained, but it often ends by making them even more complex than originally supposed. It is perhaps this tendency that leads some persons, impatient for change, to charge social scientists with being "conservative" (an otherwise hilarious accusation, given the political predispositions of most social scientists). The more complex a situation is thought to be and the greater the importance of subjective, as opposed to material, conditions in explaining it, the more intractable it will seem to be. In the long run, the Second Law tends to cover more cases than the First Law. But in the long run policy will be made whatever social science may say and, indeed, the commitment to policy change is in many cases a necessary precondition for an evaluative study.

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Irving Kristol, Editor

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CURRENT READING

Does Money Buy Family Stability?

One often hears it said that America's rising affluence and educational levels, together with increasing leisure time and expanded freedom, have conspired to put the institution of marriage under increasingly severe strain. Well, maybe so, but you certainly couldn't prove it by the Census Bureau's recent report on American marriage patterns. Indeed, if the census figures show anything, it is that the stability of marriages seems to increase with income and education.

The Bureau's survey, conducted in 1971, found that for households whose heads are from 35 to 54 years old, higher income and more schooling were associated with greater family stability. Among families earning under $5,000 a year, 71.7 per cent had been married just once. This first-marriage figure rises steadily with income level, and for families earning $15,000 or more, fully 83.0 per cent of husbands and wives were found to be in their first marriage.

The trend is even more pronounced when one looks at educational levels. Where neither husband nor wife has finished high school, both partners were found to be in their first marriage in 75 per cent of the cases; where both are college graduates, the figure stood at 90.4 per cent. And these figures emerge despite the better access which the relatively well-off presumably enjoy to the information, services, and money necessary for a divorce.

Elsewhere in this issue, Richard Easterlin argues that money does in fact buy us some personal happiness. It would seem that money and education are capable of buying us marital happiness as well—or, if not actual happiness, then at least a degree of marital stability.

Heredity and Open Inquiry

Over the past several years there have been persistent efforts to rule out of scientific inquiry the study of heredity, on the grounds that its findings might be misused. In the face of such pressures, a group of distinguished psychologists, psychiatrists, and sociologists signed a statement supporting open inquiry into these problems. The statement, which appeared in the July 1972 issue of *The American Psychologist*, describes the present situation as follows:

> The history of civilization shows many periods when scientific research or teaching was censured, punished, or suppressed for nonscientific

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reasons, usually for seeming to contradict some religious or political belief. Well-known scientist victims include: Galileo, in orthodox Italy; Darwin, in Victorian England; Einstein, in Hitler's Germany; and Mendelian biologists, in Stalin's Russia.

Today, a similar suppression, censure, punishment, and defamation are being applied against scientists who emphasize the role of heredity in human behavior. Published positions are often misquoted and misrepresented; emotional appeals replace scientific reasoning; arguments are directed against the man rather than against the evidence (e.g., a scientist is called "fascist," and his arguments are ignored).

A large number of attacks come from nonscientists, or even anti-scientists, among the political militants on campus. Other attackers include academics committed to environmentalism in their explanation of almost all human differences. And a large number of scientists, who have studied the evidence and are persuaded of the great role played by heredity in human behavior, are silent, neither expressing their beliefs clearly in public, nor rallying strongly to the defense of their more outspoken colleagues.

The results are seen in the present academy: it is virtually heresy to express a hereditarian view, or to recommend further study of the biological bases of behavior. A kind of orthodox environmentalism dominates the liberal academy, and strongly inhibits teachers, researchers, and scholars from turning to biological explanations or efforts.

Among the signers were: EDGAR F. BORGATTA, Professor of Sociology, Queens College, New York; RAYMOND B. CATTELL, Distinguished Research Professor of Psychology, University of Illinois; FRANCIS H. C. CRICK, Nobel Laureate, Medical Council Laboratory of Molecular Biology, Cambridge University; C. D. DARLINGTON, FRS, Sherardian Professor of Botany, Oxford University; OTIS DUDLEY DUNCAN, Professor of Sociology, University of Michigan; HANS J. EYSENCK, Professor of Psychology, Institute of Psychiatry, University of London; GARRETT HARDIN, Professor of Human Ecology, University of California, Santa Barbara; HARRY S. HARLOW, Professor of Psychology, University of Wisconsin; E. LOWELL KELLY, Professor of Psychology & Director, Institute of Human Adjustment, University of Michigan; QUINN MCNEMAR, Emeritus Professor of Psychology, Education, and Statistics, Stanford University; PAUL E. MEEHL, Regents Professor of Psychology and Adjunct Professor of Law, University of Minnesota; JACQUES MONOD, Nobel Laureate, Professor, Institut Pasteur, College de France; JOHN H. NORTHRUP, Nobel Laureate, Professor Emeritus of Biochemistry, University of California and Rockefeller University; DAVID ROSENTHAL, Research Psychologist and Chief of Laboratories, National Institute of Mental Health; ELIOT SLATER, M.D., Professor of Psychiatry and Editor, *British Journal of Psychiatry*, University of London; S. S. STEVENS, Professor of Psychophysics, Harvard University; ROBERT L. THORNDIKE, Professor of Psychology and Education, Teachers College, Columbia University; DAVID WECHSLER, Professor of Psychology, N.Y.U. College of Medicine.

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